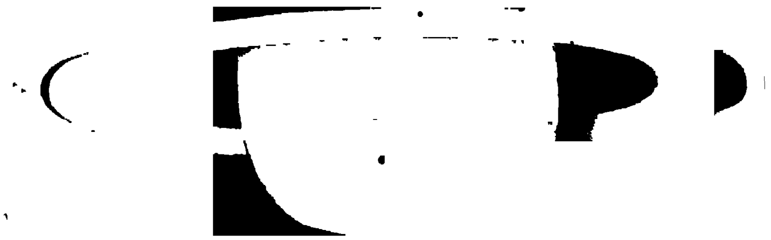
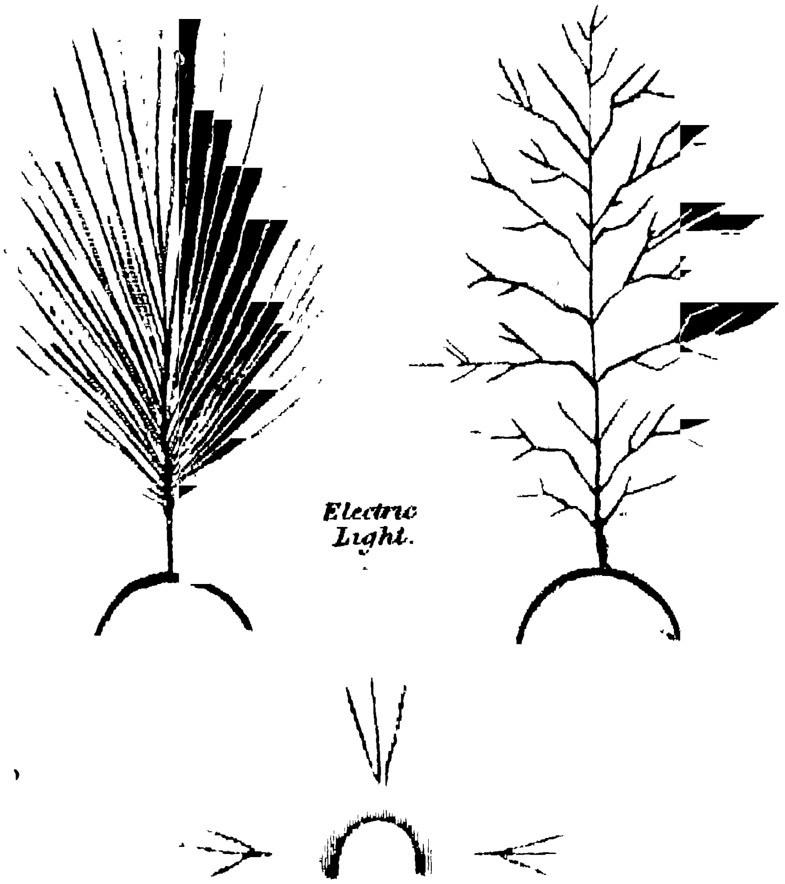
Nicholson's Philos. Journal Vol. XIII to face Title

Singular Figure of Saturn





Electric Light.

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A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. XIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1806.

PREFACE.

THE Authors of Original Papers are, J. Gough, Esq.; Dr. Beddoes; H. B.. K.; A Corespondent; J. S. Butt, Esq.; Mr. Richard Winter; H. Davy, Esq. F. R. S.; W. N.; Mr. Florian Jolly; Mr. R. Harrup; Mr. Alex. Crombie; Mr. James Stodart; A. F.; K. H. D.; Mr. J. W. Boswell; G. C.; W. Brande, Esq.; M. Cowan, Esq.; Mr. T. Northmore; Mr. J. Martin; T. Young, M. D. F. R. S.; Mr. J. Dalton; Dr. Okely; Mr. H. Steinhauer; J. Bostock, M. D.; A. T.; Amicus; A. B. C.

Of Foreign Works, M M. Callias and Co.; Colonel Skioeldebrand; M. Debue; M. Favre; Prof. Playfair; M. W. A. Cadell; M. Rosseau and Genon; M. Riffant; Professor Heeven; Lagrange; Curaudeau; Sorbie; Humboldt; Gay Lussac; Drappier; P. S. Girarey; Messrs. Reynard and Facquer; Bucholz; Hermestadt; Biemontier; M. P. Dispan; M. Poideyin; Haufman.

And of English Memoirs abridged or extracted, William Herschel, L. L. D. F. R. S.; J. Horsburgh, Esq.; Henry Cavendish, F. R. S.; C. Hatchett, Esq.; Dr. Balfour; M. Flinders, Esq.; W. H. Wollaston, M. D. Sec. R. S.; Rev. W. Gregor; Mr. W. Shirreff; Rev. Dr. W. Richardson; Benjamin Smith Barton, M. D.; Dr. Holme; Sir James Hall, Bart. F. R. S.; Mr. B. Gibson; T. A. Knight, Esq. F. R. S.; Rt. Rev. Bishop Madison; Mr. B. Gibson; Mr. Thomas Earnshaw.

Soho Square, London, May 1, 1806.

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SUPPLEMENT.

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A
JOURNAL
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NATURAL PHILOSOPHY, CHEMISTRY,
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THE ARTS.

JANUARY, 1806.

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ARTICLE I.

On the Cauſe of Fairy Rings. By JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON,

SIR, *Middleſhaw, December* 7, 1805.

Introductory remarks.

YOU publiſhed in the firſt page of your ninth volume in octavo, a letter to me from the Rev. Jonathan Wilſon, vicar of Biddulph, in which the appearance of a patch of ground recently blaſted and torn up by lightning was deſcribed. The obſervations of this ingenious and accurate gentleman promiſed to throw light on the natural hiſtory of fairy-rings, provided they were continued, and in this expectation, I took the liberty in a note ſubjoined to the copy of his letter printed by you, to requeſt his future remarks on the ſubject, drawn from repeated inſpection of the place affected by the lightning. The following is an extract of a letter from Mr. Wilſon, containing his obſervations relative to the ſubſequent appearances of the patch, with ſome thoughts which are certainly an improvement in one theory of fairy-rings, that has received the patronage of ſome writers. This letter is dated November 1, 1805, and after ſome prefatory matter, proceeds thus, Mr. Wilſon's remarks begin.

The place not eaſy to be found.

Slight veſtiges of the lightning.

" In conſequence of your eſteemed favour of the 14th of Auguſt, 1804, I went on the 2nd of September following, to view the place which the lightning had ſtruck, being accompanied by the farmer of the grounds. The affected ſpot was not then very eaſy to be diſtinguiſhed, as the injured thiſtles were generally overtopped at the time, but we had no doubt of its true ſituation, upon finding the place where we formerly dug in purſuit of the imaginary ſtone. Some dead graſs appeared, but it was confined to the ſpace within which the roots had been plowed up by the electric fluid. The verdure was undoubtedly brighter about the hole, and the farmer was willing to attribute the flouriſhing ſtate of this circle of herbitage to the lightning; but for my part, I aſcribed it to what had dropped from his cows, rather than any thing that had fallen from the clouds.

Theſe veſtiges not permanent.

" I have not been able to perceive the leaſt difference between the part ſtruck, and the reſt of the field, during the courſe of the preſent year; my obſervations muſt therefore be acknowledged not to favour the hypotheſis, which ſuppoſes fairy-rings to be formed at firſt by lightning.

The explanation by grubs improved.

" I never ſaw a fairy-ring, and therefore may ſeem badly qualified to write on the ſubject; but from what I have read, it appears to me, that the circle of decayed graſs is cauſed by the innumerable grubs, which are ſaid to lay concealed under the ring among the roots of the herbitage; I alſo ſuppoſe, that the funguſes commonly ſeen on fairy-rings, give a preference to theſe circles on account of the abundance of dead vegetable matter to be found in them; amongſt which various ſpecies of fungi are known to grow. To this I may add, that the interior circle of dark green graſs is owing to the dung, and ploughing of thoſe animals in the preceding year; and the reaſon which compels theſe grubs or their offspring to puſh forward from the centre, ſeems to be this; every creature we know of has an averſion to working in its own excrement, or that of its own ſpecies."

Reflections on theſe obſervations.

The obſervations of Mr. Wilſon, ſtated above, ſeem to demonſtrate, that a patch of herbage is not invariably converted into a fairy-ring by a powerful ſtroke of lightning; conſequently if electric diſcharges from the atmoſphere be the primary cauſes of theſe circles, they require the aſſiſtance of ſome peculiarity in the ſoil to give permanency to the appearance,

ance. Moreover the circular figure of theſe phenomena embarraſſes the electrical hypotheſis with a ſecond difficulty; for the tracts of diſcoloured graſs, actually produced by lightning, are but ſeldom bent into rings, as they more frequently aſſume a zigzag, or elſe a ramified form, and are, I believe, of but ſhort duration; which ſhews, the roots of the herbage are not deſtroyed, unleſs where the earth is torn up. The theory which attributes theſe circles of withered graſs to the running of a fungus, has little or no foundation; becauſe theſe imperfect plants, generally ſpeaking, attach themſelves to dead vegetables, conſequently their preſence in fairy-rings is nothing more than an appearance which is ſubſequent to the deſtruction of the herbage upon them. As for the lively verdure of the graſs on the interior edges of theſe circles, I believe it may be explained upon general principles, without the agency of lightning or fungi. For if the herbage of a patch of ground be deſtroyed root and branch by a cauſe which does not remove the remains of it, the place will be covered, in proceſs of time, with a freſh crop of plants, poſſeſſing ſuperior luxuriancy and verdure. The cauſes of this vigorous vegetation appear to be the following; the dead roots and ſtems rot and manure the ſoil which produced them: this ſource of fertility receives an additional ſupply from a ſucceſſion of fungi, which grow and decay on the ſurface of the ground, as well as from the excrement and exuviæ of the grubs, which take up their abode among the withered roots; laſtly, the ſoil is rendered more porous by the decay of the vegetable remains, and thereby becomes more permeable to the air, which increaſes its fertility not a little. The laſt poſition ſeems to be confirmed by the circumſtance of plants thriving better in unglazed, than in glazed flower pots. The following facts may be adduced in corroboration of what has been here advanced. A ſmall piece of ground was covered, in June, with common ſalt, which had been rubbed upon the corps of a drowned man; the herbage of this place died in a ſhort time; but was ſucceeded the next ſummer by a new crop, the verdure of which diſtinguiſhed it for ſome years from the ſurrounding graſs. As common ſalt is eſteemed a manure, perhaps the following inſtance will be called preferable to the former. Many woods in this country, eſpecially thoſe about Windermere, are cut

Difficulties of the electrical theory.

Fungi not the cauſe of fairy rings.

Superior verdure of theſe rings explained.

Dead plants, &c. manure the ground.

The air increaſes vegetation by acting on the roots.

Proofs of the preceding theory.

down about once in fourteen years, and converted into charcoal, for the uſe of the iron works. This is done by throwing the branches into large heaps; which are then covered with green turf, and ſet on fire. Theſe piles of wood continue burning for ſeveral days, in conſequence of which, the roots in the ground beneath them are completely charred; and the pit-ſtead, as the place is called, has no verdure left upon it. The loſs however is repaired in the courſe of a few years by nature herſelf, where art does not interfere, and the ſpot is clothed with a freſh coat of herbage, conſiſting of herbs remarkable for their ſize and flouriſhing appearance. This inſtance of vigorous vegetation on ground that has been completely burned, in all probability, is occaſioned by the texture of the ſoil; which is adapted to retain moiſture, and admit the air; unleſs we ſuppoſe the incorruptible ſubſtance of charcoal to afford a ſpecies of manure.

The preceding hints may perhaps incite ſome of your readers to ſtudy the natural hiſtory of fairy-rings with greater diligence, eſpecially to ſearch after the true cauſe which blaſts theſe circles; for when this is diſcovered, we ſhall be able to re-clothe them with ſuperior verdure, on rational principles.

I remain, &c.

JOHN GOUGH.

II.

Obſervations on the ſingular Figure of the Planet Saturn. By WILLIAM HERSCHEL, *LL. D. F. R. S.*[*]

Examination of the ſtriking phenomena of the planet Saturn.

THERE is not perhaps another object in the heavens that preſents us with ſuch a variety of extraordinary phenomena as the planet Saturn: a magnificent globe, encompaſſed by a ſtupendous double ring: attended by ſeven ſatellites: ornamented with equatorial belts: compreſſed at the poles: turning upon its axis: mutually eclipſing its ring and ſatellites, and eclipſed by them: the moſt diſtant of the rings alſo turning upon its axis, and the ſame taking place with the fartheſt of the ſatellites: all the parts of the ſyſtem of Saturn occaſionally reflecting light to each other: the rings and moons illuminating the nights of the Saturnian: the globe and ſatellites enlightening the dark parts of the rings: and the planet and rings throwing back the ſun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

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[*] Philoſophical Tranſactions, 1805.

It muſt be confeſſed that a detail of circumſtances like theſe, appears to leave hardly any room for addition, and yet the following obſervations will prove that there is a ſingularity left, which diſtinguiſhes the figure of Saturn from that of all the other planets.

Its body oblate.

It has already been mentioned on a former occaſion, that ſo far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781 that it was flattened at the poles at leaſt as much as Jupiter, I was inſenſibly diverted from a more critical attention to the reſt of the figure. Prepoſſeſſed with its being ſpheroidical, I meaſured the equatorial and polar diameters in the year 1789, and ſuppoſed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally aſcribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might ſtill have remained, had not a late examination of the powers of my ten-feet teleſcope convinced me that I ought to rely with the greateſt confidence upon the truth of its repreſentations of the moſt minute objects I inſpected.

Reaſons why the greater peculiarities of its figure were overlooked:

The following obſervations, in which the ſingular figure of Saturn is fully inveſtigated, contain many remarks on the reſt of the appearances that may be ſeen when this beautiful planet is examined with attention; and though they are not immediately neceſſary to my preſent ſubject, I thought it right to retain them, as they ſhow the degree of diſtinctneſs and preciſion of the action of the teleſcope, and the clearneſs of the atmoſphere at the time of obſervation.

Very perfect obſervation, in which the circumference was ſeen to be flattened in four regions.

April 12, 1805. With a new 7-feet mirror of extraordinary diſtinctneſs, I examined the planet Saturn. The ring reflects more light than the body; and with a power of 570 the colour of the body becomes yellowiſh, while that of the ring remains more white. This gives us an opportunity to diſtinguiſh the ring from the body, in that part where it croſſes the diſk, by means of the difference in the colour of the reflected light. I ſaw the quintuple belt, and the flattening of the body at the polar regions; I could alſo perceive the vacant ſpace between the two rings.

Obſervations on Saturn by which its ſingular figure is aſcertained.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it ſeems not to begin till at a high latitude, and there to be more ſudden than it is towards the poles of Jupiter. I have often made the ſame obſervation before, but do not remember to have recorded it any where.

April 18; ten-feet reflector, power 300. The air is very favourable, and I ſee the planet extremely well defined. The ſhadow of the ring is very black in its extent over the diſk ſouth of the ring, where I ſee it all the way with great diſtinctneſs.

The uſual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be ſeen in the figure I have given in Plate I.; and alſo in a former repreſentation of the ſame belts in 1794.*

The figure of the body of Saturn, as I ſee it at preſent, is certainly different from the ſpheroidical figure of Jupiter. The curvature is greateſt in a high latitude.

I took a meaſure of the ſituation of the four points of the greateſt curvature, with my angular micrometer, and power 527. When the croſs of the micrometer paſſed through all the four points, the angle which gives the double latitude of two of the points, one being north the other ſouth of the ring, or equator, was 93° 16′. The latitude therefore of the four points is 46° 38′; it is there the greateſt curvature takes place. As neither of the croſs wires can be in the parallel, it makes the meaſure ſo difficult to take, that very great accuracy cannot be expected.

The moſt northern belt comes up to the place where the ring of Saturn paſſes behind the body, but the belt is bent in a contrary direction being concave to the north, on account of its croſſing the body on the ſide turned towards us, and the north pole being in view.

There is a very dark, but narrow ſhadow of the body upon the following part of the ring, which as it were cuts off the ring from the body.

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* See Phil. Tranſ. for 1794, Table VI. page 32.

Obſervations on Saturn, by which its ſingular figure is aſcertained.

The ſhadow of the ring on the body, which I ſee ſouth of the ring, grows a little broader on both ſides near the margin of the diſk.

The diviſion between the two rings is dark, like the vacant ſpace, between the anſæ, but not black like the ſhadow I have deſcribed.

There are four ſatellites on the preceding ſide near the ring; the largeſt and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet teleſcope, both mirrors of which are very perfect. I ſaw all the phenomena as deſcribed laſt night, except the ſatellites, which had changed their ſituation; four of them being on the following ſide. This teleſcope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we ſee things at firſt ſight that would otherwiſe have eſcaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is ſomewhat like a ſquare or rather parellelogram, with the four corners rounded off deeply, but not ſo much as to bring it to a ſpheroid. I ſee it in perfection.

The four ſatellites that were laſt night on the preceding, are now on the following ſide, and are very bright.

I took a meaſure of the poſition of the four points of the greateſt curvature, and found it 91° 29′. This gives their latitude 45° 44′,5. I believe this meaſure to be pretty accurate. I ſet firſt the fixed thread to one of the lines, by keeping the north-preceding and ſouth-following two points in the thread; then adjuſted the other thread in the ſame manner to the ſouth-preceding and north-following points.

May 5, 1805. I directed my 20-feet teleſcope to Saturn, and, with a power of about 300, ſaw the planet perfectly well defined, the evening being remarkably clear. The ſhadow of the ring on the body is quite black. All the other phenomena are very diſtinct.

The figure of the planet is certainly not ſpheroidical, like that of Mars and Jupiter. The curvature is leſs on the equator

and

Obſervations on Saturn, by which its ſingular figure is aſcertained.

and on the poles than at the latitude of about 45 degrees. The equatorial diameter is however conſiderably greater than the polar.

In order to have the teſtimony of all my inſtruments, on the ſubject of the ſtructure of the planet Saturn, I had prepared the 40-feet reflector for obſerving it in the meridian. I uſed a magnifying power of 360, and ſaw its form exactly as I had ſeen it in the 10 and 20-feet inſtruments. The planet is flattened at the poles, but the ſpheroid that would ariſe from this flattening is modified by ſome other cauſe, which I ſuppoſe to be the attraction of the ring. It reſembles a parallelogram, one ſide whereof is the equatorial, the other the polar diameter, with the four corners rounded off ſo as to leave both the equatorial and polar regions flatter than they would be in the regular ſpheroidical figure.

The planet Jupiter being by this time got up to a conſiderable altitude, I viewed it alternately with Saturn in the 10-feet reflector, with a power of 500. The outlines of the figure of Saturn are as deſcribed in the obſervation of the 40-feet teleſcope; but thoſe of Jupiter are ſuch as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-feet teleſcope of 24 inches aperture; and ſaw plainly that the former planet differs much in figure from the latter.

The temperature of the air is ſo changeable that no large mirror can act well.

May 13. 10-feet reflector, power 300. The ſhadow of the ring upon the body, and of the body upon the ring, are very black, and not of the duſky colour of the heavens about the planet, or of the ſpace betwen the ring and planet, and between the two rings. The north-following part of the ring, cloſe to the planet, is as it were cut off by the ſhadow of the body; and the ſhadow of the ring lies ſouth of it, but cloſe to the projection of the ring.

The planet is of the form deſcribed in the obſervation of the 40-feet teleſcope; I ſee it ſo diſtinctly that there can be no doubt of it. By the appearance, I ſhould think the points

Obſervations on Saturn, by which its ſingular figure is aſcertained.

points of the greateſt curvature not to be ſo far north as 45 degrees.

The evening being very calm and clear, I took a meaſure of their ſituation, which gives the latitude of the greateſt curvature 45° 21′. A ſecond meaſure gives 45° 41′

Jupiter being now at a conſiderable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is leſs than on Saturn.

I repeated theſe alternate obſervations many times, and the oftener I compared the two planets together, the more ſtriking was their different ſtructure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two meaſures of the diameter of the points of greateſt curvature. A mean of them gave 64,3 diviſions = 11″,98. After this, I took alſo two meaſures of the equatorial diameter, and a mean of them gave 60,5 diviſions = 11″,27; but the equatorial meaſures are probably too ſmall.

To judge by a view of the planet, I ſhould ſuppoſe the latitude of the greateſt curvature to be leſs than 45 degrees. The eye will alſo diſtinguiſh the difference in the three diameters of Saturn. That which paſſes through the points of the greateſt curvature is the largeſt; the equatorial the next, and the polar diameter is the ſmalleſt.

May 27. The evening being very favourable, I took again two meaſures of the diameter between the points of greateſt curvature, a mean of which was 63,8 diviſions = 11″,68. Two meaſures of the equatorial diameter gave 61,3 diviſions = 11″,44.

June 1. It occurred to me that a more accurate meaſure might be had of the latitude in which the greateſt curvature takes place, by ſetting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following meaſures were taken in this manner, and are more ſatisfactory than I had taken before. The firſt gave the latitude of the ſouth-preceding point of greateſt curvature 43° 26′; and the ſecond 43° 13′. A mean of the two will be 43° 20′.

June

Obſervations on Saturn, by which its ſingular figure is aſcertained.

June 2. I viewed Jupiter and Saturn alternately with a magnifying power of only 300, that the convexity of the eye-glaſs might occaſion no deception, and found the form of the two planets to differ in the manner that has been deſcribed.

With 200 I ſaw the difference very plainly; and even with 160 it was ſufficiently viſible to admit of no doubt. Theſe low powers ſhow the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in paſſing is leſſened, we are more at liberty to fix our attention upon them.

I compared the teleſcopic appearance of Saturn with a figure drawn by the meaſures I have taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789;* and found that, in order to be a perfect reſemblance, my figure required ſome ſmall reduction of the longeſt diameter, ſo as to bring it nearly to agree with the meaſures taken the 27th of May. When I had made the neceſſary alteration, my artificial Saturn was again compared with the teleſcopic repreſentation of the planet, and I was then ſatisfied that it had all the correctneſs of which a judgment of the eye is capable. An exact copy of it is given in Plate IX. The dimenſions of it in proportional parts are,

The diameter of the greateſt curvature -	36
The equatorial diameter - - - -	35
The polar diameter - - - - -	32
Latitude of the longeſt diameter -	43° 20.′

The foregoing obſervations of the figure of the body of Saturn will lead to ſome intricate reſearches, by which the quantity of matter in the ring, and its ſolidity, may be in ſome meaſure aſcertained. They alſo afford a new inſtance of the effect of gravitation on the figure of planets; for in the caſe of Saturn, we ſhall have to conſider the oppoſite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been aſcertained in ſome of my former Papers.

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* See Phil. Tranſ. for 1790, page. 17.

III.

Facts and Obſervations on the medical Reſpiration of gazeous Oxide of Azote. In a Letter from Dr. BEDDOES.

To Mr. NICHOLSON.

SIR,

Dr. Pfaff's experiments on reſpiration.

Propoſes gaſeous oxide of azote in melancholy madneſs.

DR. Pfaff's paper on reſpiration* will probably draw the attention of the ſcientific towards the gaſeous oxide of azote, which has been too much neglected in a medical point of view. I was only ſorry to ſee that he propoſes to uſe it in melancholia. No combination of ideas can be more obvious than the application of an agent which has ſo frequently proved exhilarating, and never yet been obſerved to be followed by exhauſtion where it did exhilarate, to a complaint, in which depreſſion of ſpirits is a ſtriking circumſtance. But I am apprehenſive that the firſt thoughts of inexperience here (as ſo often happens) will prove illuſory, and that this project will not be followed by the expected advantage in many caſes of melancholia. For if it be true that there is no real diſtinction between mania and melancholia, as far as the ſenſorium is concerned, and that the vivacity of ideas in melancholia anſwers to the violence of muſcular actions in mania, as I have endeavoured to ſhew in my *Eſſays on Health*; is there not ground to apprehend that the actions of the brain, already too ſtrong, will be increaſed by this gas, or the diſeaſed contemplations rendered more intenſe?

Cautions againſt its unguarded uſe, from theory;

If there be any ſtate of melancholia in which it may be of ſervice, this will probably happen when the nervous ſyſtem is falling into debility, in conſequence of having been kept too much on the ſtretch.

from experience.

Caſes.

But I do not here warn againſt gaſeous oxide from mere theory. The manager of a lunatic aſylum near Briſtol, reſpectably known to the public, concurred with me ſome years ago in the opinion which I expreſſed to him concerning its probable advantage in melancholia; and a patient that had been under his care inhaled it fairly without benefit. The adminiſtration was tried in two other caſes as fruitleſsly: Indeed

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* Philoſ. Journal, XII, 249.

I diſcontinued it in one, from ſome indications of an aggravation of the ſymptoms. I was by this time alive to ſuſpicion, having thought much on the ſubject, and reaſoned myſelf into the idea that it would often do injury upon the above-mentioned principle. It has long been my opinion, and there are ſtriking obſervations on record to prove that hidrogen, hidro-carbonate, azotic, or carbonic acid gaſes, would be more likely to anſwer in active inſanity under whatever form. Theſe obſervations I ſhall take occaſion to quote hereafter.

Uſe in palſy of one kind.

The very firſt time I witneſſed the effects of gaſeous oxide on a perſon in health, I concluded that it would be a remedy in certain caſes of palſy. A patient who had emerged from apoplexy with the loſs of the power of one ſide of his body, was accordingly put under a courſe of the gas. The reſult completely anſwered expectation. The caſe was moſt carefully watched; and on withholding the gas, the ſymptoms repeatedly grew worſe, and *vice verſa*. After the patient's recovery, he was kept under inſpection for a conſiderable time, and did not relapſe. This has been confirmed by other reſults; and in palſy, where the brain is primarily affected, I expect that Dr. Pfaff will find either a cure or great relief to follow the uſe of this gas in a reſpectable proportion of caſes.

In another kind of palſy.

Of oxigen,

I have very fairly tried it in palſy apparently from cold, beginning at the extremities and creeping from muſcle to muſcle, without good or bad effect. There is a caſe of this kind, related by Dr. Kentiſh, with the patient's name, and corroborated by teſtimony ſuperior to all exception in *Conſiderations on factitious Airs (Johnſon)* in which a perfect cure was obtained from oxigen gas; and I have ſince learned, by experiments carefully repeated before various philoſophical obſervers, that *in eſſential reſpects, oxigen gas and gaſeous oxide act in a very different, nay oppoſite manner upon the living fibre.*

Theſe experiments I hope to publiſh before midſummer.

In dropſies;

From palſy, analogy led me to other caſes of debility. I fully tried gaſeous oxide in dropſy of the cheſt (anaſarca of the lungs), but without good or bad effect. I was much diſappointed, conceiving that in dropſy (at leaſt in one ſpecies) we have a paralytic ſtate of the lymphatics. But I have been ſince aſſured by a phyſician, that for ſome dropſies he has found a remedy in this gas. There are dropſies which doubtleſs depend on exceſs of exhalant action. Theſe are eaſily diſtinguiſhed; and they require bleeding as much as pleuriſy.

In other ſtates of debility.

In debility, ariſing from reſidence in hot climates and from intenſe application to buſineſs, I have known gaſeous oxide completely ſucceſsful after an infinity of remedies, Bath and other waters, had been tried in vain.

The particulars of theſe caſes are alſo deſtined for publication: But I reſolved to wait for ſome years after the uſe of the gas; for I have found that a ſingle circumſtance vitiates a large proportion of our medical records. Patients after an apparent recovery fall again into the ſame complaint; and there are other conſiderations, which I ſhall for the preſent paſs over.

Gaſeous oxide has given voluntary power over palſied parts, while inhaled.

If Mr. Pfaff uſes gaſeous oxide in palſy, he will probably ſooner or later ſee a phenomenon as extraordinary as any in galvaniſm, and which after it has been deſcribed by a philoſopher of high reputation, will become equally celebrated. This is the inſtantaneous reſtoration of voluntary power over a limb deprived both of motion and feeling by palſy ſucceeding to apoplexy, while the patient is reſpiring gaſeous oxide. This was witneſſed in common with myſelf, by ſeveral reſpectable perſons; and among others by ſome of your philoſophical acquaintance, if I do not miſtake. It was in the caſe of Mr. G. a member of the laſt parliament, who completely recovered: But as other means were afterwards adopted, I do not impute the reſult to the gas, which however, when uſed alone, was viſibly of great ſervice: for I have no idea of claiming for a remedy under ſcrutiny any cure, if other powers have been called in at the ſame time.

Firſt wiſhed for effect of this ſtatement.

I tranſmit theſe obſervations to you, Sir, in preference to the Editor of any Medical Journal, becauſe I think them likely to meet the eye of Dr. Pfaff ſooner in your Journal. I ſhould be extremely ſorry that he ſhould ſet out wrong in his trials, becauſe the fault will be imputed to the power itſelf, and not to its miſapplication; and the diſabled will ſtill be left to languiſh and be cut off, notwithſtanding we have a remedy at hand.

A ſecond.

I have another reaſon. I moſt ſincerely wiſh any thing I could ſay would haſten the period, which *muſt* arrive, when medical ſcience ſhall not be merely what the Germans call a

Brod-

Men of liberal curioſity exhorted to ſtudy medicine.

Brod-wiſſenſchaft, or purſued only for a livelihood. If philoſophical men without a profeſſion would take it up, it is I think certain, that it muſt ſoon become both more efficient and more liberal. Any ſtudy is capable of intereſting the feelings; and moſt ſurely that of the laws of the organic world is as much ſo as any other. Opportunities of anatomical, chemical, and *clinical* information are at hand. A perſon ſo prepared will, heaven knows, with ardour and induſtry ſoon acquire all that is uſeful in medical practice. Let him then, animated with no other motive than the pure deſire of benefiting his fellow men, apply himſelf to the improvement of medicine. It is impoſſible that he ſhould not ſucceed as fully as our Tennants, our Hatchetts, and Chenevix's have done in chemiſtry; for it is not its inherent difficulty, but collateral circumſtances, that retard the progreſs of this art. Many apothecaries, for example, and old women in general, who are the great controulers of the deſtiny of phyſicians, would by no means allow the uſe of gaſeous oxide in palſy, though the patient in the courſe both of nature and of ordinary medication be ſure to die, and perhaps in a very miſerable manner. But the philoſophical cultivator of medicine, without troubling himſelf about the good opinion of the one or the other, would proceed on his career under the guidance of the collective light of ſcience and of humanity.

Anecdote.

N. N. advanced in years, of a thickſet ſtature, and with a ſhort neck, ſhewed ſigns of palſy many years ago. The writer of theſe lines warned his friends of the danger. Concurring in this apprehenſion, Dr. Ingenhouſz propoſed to him to inhale oxigen gas, a practice familiar to that accurate philoſopher, and by which he hoped the conſtitution might be recruited. The execution of the idea was deferred. Meanwhile the gaſeous oxide was diſcovered to be reſpirable, and its power in palſy was to a degree aſcertained. The writer now preſſed the uſe of this gas with the utmoſt earneſtneſs. The patient ſaw it taken by others: He himſelf conſented to inhale it, when behold! the diſtreſs of a lady preſent, as excited by ſome apprehended imaginary bad conſequences, put off the inhalation. The predicted paralytic ſeizure arrived: but there was ample time ſtill for the uſe of the oxide. I propoſed that another patient, ſituated as ſimilarly as poſſible, ſhould be ſought; and that if he conſented upon the credit of the ſucceſsful exhibition, and upon my reſponſibility, to uſe the gas, the reſult ſhould determine as to its employment in the caſe firſt in queſtion. At the ſame time, I ſtated from the average courſe of paralytic attacks in general not immediately fatal, that a little apparent amendment would take place, and the ſtroke return with additional violence. My propoſal was acknowledged to be highly reaſonable; but that plan of routine treatment was followed which is ſo much more advantageous to the idle and unſcientific of our profeſſion than it is to the ſick, and the patient died of a return of his complaint. Such is probably the condition of thouſands of the diſeaſed at this moment! Rather than uſe a recently propoſed plan not in the Pharmacopœia, or ſeek a new one in analogy, we perſevere in painful or diſguſting means, from which, on the faith of long experience, no good of any ſort can be expected for the ſufferer. May the riſing generation of natural philoſophers exerciſe their talents and their benevolence in putting an end to ſo crying an evil!

I am, Dear Sir,

Reſpectfully your's,

THOMAS BEDDOES.

Clifton, Dec. 13, 1805.

On effects of gaſ. ox. as ſtated in this Journ.

P. S. A caſe in your Journal, where a gentleman accuſtomed to breathe gaſeous oxide for amuſement, experienced very diſagreeable feelings on one particular occaſion, ſeems to me clearly referable to hyſteria. Now the trials at the Pneumatic Inſtitution, as related in Mr. Davy's *Reſearches*, had clearly ſhewn that in the prediſpoſed, gaſeous oxide is a ſpecific for exciting an hyſteric paroxyſm. Perhaps in the individual whoſe caſe is related by himſelf in the Journal, no obvious prediſpoſition, either temporary or permanent, exiſted: Nothing to this purport is ſtated. But that the affection was ſimply hyſterical cannot I think be doubted by any one converſant both with hyſteria and the adminiſtration of gaſeous oxide. It ſeems to be ſtrongly marked by that idea of immediate danger, which is ſo common in hyſteria. Dr. Garnet very unneceſſarily, and, I believe, very miſtakenly, called up the whole Brunonian theory on the emergency. It led him, however, to give cordials; and they were proper. A tea-ſpoonful of ſal volatile,

Their real nature.

from

Caution regarding particular ſubjects.
Quære.

from time to time, would probably have anſwered without the Brunonian theory. But it is certainly the buſineſs of the phyſician to avoid gaſeous oxide in the hyſterical, as it is wine in thoſe who labour under acute inflammation. If your correſpondent who related his own feelings could ſpecify any cauſe which might have rendered him nervous, or ſtate the fact whether he was ſo or not, it would give ſatisfaction to the preſent writer, and perhaps alſo to future inquirers.

Remark on dangerous diſorders.

To interdict a remedy becauſe its uſe requires diſcrimination, would, in many diſorders, be leaving the ſick to certain deſtruction. I imagine that the outcry againſt ſuch means as gaſeous oxide, will ariſe from thoſe who daily uſe the moſt hazardous remedies, and who are enabled to do it without reproach, becauſe they are put into a phial, and the patient and his friends never trouble themſelves about the nature of the articles which they are receiving into the ſtomach.

IV.

Abſtract of Obſervations on a diurnal Variation of the Barometer between the Tropics. By J. HORSBURGH, *Eſq. In a Letter to* HENRY CAVENDISH, *Eſq. F. R. S.** *Read March* 14, 1805.

SIR, *Bombay, April,* 20, 1804.

Tropical variation of the barometer.

WHEN I was in London at the concluſion of the year 1801, I had the pleaſure of being introduced to you by my friend Mr. Dalrymple, at which time he preſented you with ſome ſheets of meteorological obſervations, with barometer and thermometer, made by me in India, and during a paſſage from India to England.

Being of opinion that few regiſters of the barometer are kept at ſea, eſpecially in low latitudes, I have been induced to continue my obſervations ſince I left England, judging that, even if they were found to be of no utility, they might at leaſt be entertaining to you or other gentlemen, who have been making obſervations of a ſimilar nature.

During my laſt voyage I have employed two marine barometers, one made by Troughton, the other by Ramſden,

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* Philoſophical Tranſactions, 1805.

Tropical variation of the barometer.

and a thermometer by Frazer. Theſe were placed expoſed to a free current of air in a cabin, where the baſons of the barometers were 13 feet above the level of the ſea.

The hours at which the heights of the barometers, and thermometers were taken, *viz.* noon, 4 hours, 10 hours, 12 hours, 14 hours, and 19 hours, were choſen, becauſe at theſe times the mercury in the barometer had been perceived to be regularly ſtationary between the tropics, by former obſervations made in India in 1800 and 1801. It was found that in ſettled weather in the Indian ſeas, from 8 A M to noon, the mercury in the barometer was generally ſtationary, and at the point of greateſt elevation; after noon it began to fall, and continued falling till 4 afternoon, at which time it arrived at the loweſt point of depreſſion. From 4 or 5 P M the mercury roſe again, and continued riſing till about 9 or 10 P M, at which time it had again acquired its greateſt point of elevation, and continued ſtationary nearly till midnight; after which it began to fall, till at 4 A M it was again as low as it had been at 4 afternoon preceding; but from this time it roſe till 7 or 8 o'clock, when it reached the higheſt point of elevation, and continued ſtationary till noon.

Thus was the mercury obſerved to be ſubject to a regular elevation and depreſſion twice in every 24 hours in ſettled weather; and the loweſt ſtation was obſerved to be at about 4 o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low ſtation, but had a regular tendency to riſe from thence till towards 8 in the morning and about 9 in the evening, and from thoſe times continued ſtationary till noon and midnight.

In unſettled blowing weather, eſpecially at Bombay during the rains, theſe regular ebbings and flowings of the mercury could not be perceived; but a tendency to them was at ſome times obſervable when the weather was more ſettled.

In the ſheets, which I formerly preſented to you, were evinced theſe elevations and depreſſions twice every 24 hours within the tropics, in ſteady weather, as had been obſerved by Meſſ. Caſſan and Peyrouſe, by Dr. Balfour of Calcutta, and others. But ſince my laſt arrival in India, I have obſerved that the atmoſphere appears to produce a different effect on the barometer at *ſea* from what it does on *ſhore.*

Tropical variation of the barometer.

As I am ignorant whether this phenomenon has been noticed by any perſon before, I will here give you an abſtract of my journal, ſhewing how the barometer has been influenced during the whole time ſince I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at ſea from what it is on ſhore, at thoſe places in India where the obſervations have been made.

The firſt ſheet begins with the obſervations made on board ſhip, in my voyage from London towards Bombay, in the months of April and May, 1802.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° N, longitude 20° W, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depreſſions every 24 hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° N to latitude 10° N, the difference of the high and low ſtations of the mercury in the barometers was not ſo great, as it was from latitude 10° N acroſs the equator, and from thence to latitude 25° S. Within theſe laſt-mentioned limits, the difference of high and low ſtations of the mercury in the barometers was very conſiderable, generally from five to nine hundred parts of an inch, both in the daily and nightly motions.

When we reached the latitude of 28° S, longitude 27° W, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its riſing and falling became irregular and fluctuating during our run from latitude 28° S, longitude 27° W, (moſtly between the parallels of 35° and 36° S,) until we were in latitude 27° S, and longitude 51° E, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly, during our run from the laſt-mentioned poſition, up the Madagaſcar Archipelago, acroſs the Equator, until our arrival at Bombay July 31ſt. 1802.

Auguſt 6th, 1802. When the barometers were placed on ſhore in Bombay, the mercury, for the firſt ſix days, appeared to have a ſmall tendency towards performing the equatropical motions, but not equally perceptible as when at ſea, the difference between the high and low ſtations of the mercury in the

Tropical variation of the barometer.

the barometers being great to the day we entered the harbour of Bombay. From the 12th of Auguſt to the 22d the mercury could not in general be obſerved to have any inclination to perform the equatropical motions, although at times a very ſmall tendency towards performing them might be perceived.

On the 23d of Auguſt the barometers were taken from the ſhore to the ſhip. Immediately on leaving Bombay harbour, Auguſt 26th, 1802, the mercury in the barometers performed the equatropical motions, and continued them with great uniformity, during our paſſage down the Malabar coaſt, acroſs the bay of Bengal, in the Strait of Malacca, and through the China Sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly ſtationary during the 24 hours, except a ſmall inclination at times towards the equatropical motions, but they were not near ſo perceptible as at ſea; this change taking place the day we got into the river.

During our ſtay in China, the barometer on ſhore, at Canton, had very little tendency towards the equatropical motions, throughout the months of October and November that we remained there. At times, while in China, a ſmall inclination towards performing the equatropical motions appeared: but, as in Bombay, the difference of riſe and fall was of ſo ſmall a quantity, as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river, the equatropical motions were inſtantly performed by the mercury, and with great regularity continued during the whole of the paſſage to Bombay, until our arrival in that harbour on the 11th of January, 1803.

On January 18th, the barometers were placed on ſhore, and did not appear in the ſmalleſt degree ſubject to the equatropical motions; although, with great regularity, they had been performed while at ſea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on ſhore, ſeemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly ſtationary throughout the 24 hours. But about the latter part of March the mercury ſeemed to incline towards the equatropical mo-

Tropical variation of the barometer.

tions in a very ſmall degree; and, during the month of April, and to the 20th of May, this ſmall tendency of the mercury to perform the motions appeared at times, but was hardly diſcernible, the riſe and fall being of ſo ſmall a quantity. From the 18th of January to the 20th of May, the mercury in the barometers was in general ſtationary, except a very ſmall tendency towards the equatropical motions at times. At other times ſome change in the atmoſphere diſturbed the mercury from its ſtationary poſition; but this was ſeldom the caſe, as it was then the fair weather ſeaſon, or north-eaſt monſoon.

We ſailed from Bombay on the 23d of May, 1803. The inſtant we got out of the harbour, the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low ſtations was very conſiderable during the whole of the paſſage to China, excepting a few days in the eaſtern parts of Malacca Strait, where the land lay contiguous on each ſide of us; the difference between the high and low ſtations of the mercury was not then ſo great as in the open ſea. On clearing the Strait, and entering the China Sea, the equatropical motions were performed in greater quantity, and continued regular during our paſſage up the China Sea, until July 2, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceaſed.

From July 8th to September 7th, the barometers were placed on ſhore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a ſtationary poſition, except when influenced by changes of weather. After the barometers were taken from Canton to the ſhip, we were four days in getting clear of the river, in which time the mercury inclined to be ſtationary, excepting that a ſmall inclination towards the equatropical motions ſeemed to evince itſelf at times. But no ſooner had we cleared Canton river, September 13, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depreſſions every 24 hours, at equal intervals of time, (although we were near the land until the 15th September.) And the mercury, with great regularity, continued to per-

Tropical variation of the barometer.

form the equatropical motions, from September 13, 1803, the day we cleared the river of Canton, until October 13, when we entered Sincapore Strait, excepting a ſmall degree of irregularity, which affected the mercury on the 22d September, when it blew a gale on the coaſt of Iſiompa.

October 13, 1803. On entering the Strait of Sincapore, which is about 3½ leagues wide, the mercury in the barometers was then a little obſtructed, and did not perform the equatropical motions, in the ſame quantity of riſe and fall, as when we were in the China Sea. But on the following day, October 14, when we had paſſed the narrow part of the Strait, the mercury conformed to thoſe motions with regularity until October 21, when we arrived in the harbour of Prince of Wales's Iſland; then a great retardation took place in the equatropical motions; for, during the time the ſhip remained in the harbour, from October 20 to November 5, 1803, the mercury in barometers ſeemed only in a ſmall degree ſubject to them, the difference between the high and low ſtations of the mercury, being in general not more than half the quantity, that takes place in the open ſea, or at a conſiderable diſtance from land. Where the ſhip lay at this time in the harbour, the land, on one ſide, was a full quarter of a mile diſtant, and on the other ſide about 1½ mile.

On November 5, being clear of the harbour of Prince of Wales's Iſland, the equatropical motions were inſtantly performed by the mercury, in the uſual quantity experienced at ſea, which continued with uniformity until December 3. On this and the following day, the mercury fell conſiderably during our paſſage over the tails of the ſands at the entrance of Hoogly river, in latitude 21° 06′ N; and on December 5, the day of the moon's laſt quarter, a gale of wind commenced from N N E, with much lightning and rain in the night. During the latter part of this day, the mercury began to riſe, and there ſoon followed a change of ſettled weather. When we were in the lower part of the river, the mercury appeared to conform in a ſmall degree to the equatropical motions; but when well up the river, at Diamond Harbour, the mercury inclined to be nearly ſtationary during the 24 hours, as has formerly been obſerved to happen in Canton river, Bombay harbour, &c.

On

Tropical variation of the barometer.

On January 13, 1804, after we had cleared the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the paſſage to Bombay, until our arrival there on February 12. The barometers being then placed on ſhore, the mercury inclined to a ſtationary poſition, without evincing any propenſity towards the equatropical motions from the 12th to the 18th February, 1804, as has been noticed in the foregoing deſcription, to happen frequently, on entering a harbour from ſea.

On February 18, 1804, the meteorological journal ceaſes, at which time it compriſes the obſervations of 22 months, having commenced April 6, 1802, in Margate Road.

I have taken the liberty of ſending you this abſtract from the journal, to exhibit the apparent difference of the mercury in the barometer at ſea, from what has been obſerved on ſhore, at thoſe places mentioned in the preceding deſcription. As I have not ſeen any account indicating the phenomenon, I thought it might be intereſting to you, or other gentlemen of the Royal Society to forward this imperfect abſtract, the journal itſelf being too cumberſome to ſend home at preſent. But as I am in expectation of returning to England by the ſhips from China next ſeaſon, I hope I ſhall be enabled to preſent you with the meteorological ſheets alluded to.

I am, &c.

J. HORSBURGH.

P. S. Since I wrote the foregoing abſtract, I have received a letter from my friend Mr. Dalrymple, intimating that a copy of the meteorological journal itſelf would be acceptable, which has induced me to tranſmit to him the original ſheets, with a requeſt to deliver them to you. I regret that I could not find leiſure time to make out a fair copy, to have ſent to you, in place of the original ſheets in their rough ſtate.

Bombay, June 1, 1804.

V.

Second Communication on Artificial Tan. By CHARLES HATCHETT, *Eſq. Abridged from the Philoſophical Tranſactions for* 1805.

§ I.

Name of the artificial tanning ſubſtance altered.

THE artificial tan *, procured as deſcribed in the firſt communication (*ſee our Vol. XII. p.* 327), had been named tannin by Mr. Hatchett; but the objection having being made to this, that tannin was deſtroyed by the nitric acid, while the artificial tanning ſubſtance was actually formed by it, induced Mr. Hatchett to expunge the word *tannin* wherever it had been applied to the latter. It alſo induced the author to make the following experiments on the comparative effects produced by nitric acid on thoſe ſubſtances which contain moſt tannin, and alſo ſome others in which a tanning ſubſtance has been produced, under circumſtances in ſome reſpects different from thoſe deſcribed.

§ II.

Although it is not abſolutely aſſerted that the tanning ſubſtance is indeſtructible by nitric acid, yet the following experiments prove, that to produce this effect muſt at leaſt be the work of much time and difficulty.

Experiments to prove that the artificial tan is nearly indeſtructible by nitric acid.

1. Twenty grains of the artificial tan were diſſolved in half an ounce of ſtrong nitric acid, of the degree of 1.40; the ſolution diſtilled till the whole of the acid came over, which acid was returned back on the reſiduum, and the diſtillation repeated three times in this manner. Care was taken not to overheat the reſiduum; and then, when examined, did not appear to have ſuffered any alteration in its properties.

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* In ſeveral parts of the abridgement of Mr. Hatchett's papers, the artificial tanning ſubſtance has been called *the new tan* and *artificial tan*, and tanning matter *tan*, for the ſake of brevity. It was thought neceſſary to mention this, as the name tan is uſually appropriated to oak bark in a certain ſtate; which, with ſingular impropriety, is that in which it contains leaſt tanning matter, after having been uſed in the tanners' pits.—ABR.

2. Ten

Experiments continued.

2. Ten grains of the new tan, mixed with ten grains of white ſugar, diſſolved in half an ounce of nitric acid, was diſtilled to dryneſs. The reſiduum was not changed by the gelatinous or other re-agents.

3. This experiment was the ſame as the former, only that gum arabic was employed in place of ſugar. The reſult was the ſame.

4. The precipitate from a ſolution of iſinglaſs, with which the artificial tan had been mixed, was well waſhed with diſtilled water and then dried. In this ſtate it was digeſted in ſtrong nitric acid, by which a dark-brown ſolution was formed; which was evaporated to dryneſs, and the ſubſtance, diſſolved in boiling diſtilled water, was examined by nitrate of iron, acetite of lead, muriate of tin, and ſolution of iſinglaſs, with all of which it threw down copious precipitates, ſimilar in all reſpects to the artificial tan, which had not been ſubjected to the proceſs deſcribed.

5. Some of the precipitate of iſinglaſs by the new tan was diſſolved in muriatic acid, and evaporated to dryneſs: of this boiling diſtilled water diſſolved only a part; and the ſolution, of a dark beer colour, did not precipitate gelatine, though it acted on muriate of tin and ſulphate of iron; for with the former it gave an aſh-coloured precipitate, and with the latter a ſlight depoſit of a reddiſh-brown.

6. As boiling water diſſolved only a part of the iſinglaſs precipitate in the former experiment, the remainder was treated with nitric acid; after which, on being evaporated to dryneſs, it was found to be completely ſoluble in water, and precipitated gelatine as copiouſly as at firſt.

7. Twenty grains of the new tan was diſſolved in half an ounce of muriatic acid: The reſiduum, after evaporation to dryneſs, appeared in every reſpect unchanged.

The author here makes the obſervation, mentioned at the concluſion of the former paper, relative to the ſolutions of the new tan not becoming mouldy like thoſe of galls, ſumach, and catechu, and ſeeming to be completely imputreſcible.

And having thus aſcertained the unchangeable nature of this ſubſtance, he made the following comparative experiments on galls, ſumach, Pegu cutch, kaſcatti, common cutch, and oak bark.

Experiments on galls, ſumach, cutch, kaſcatti, Pegu cutch, and oak bark.

8. Twenty grains of powdered galls were diſſolved in half an ounce of ſtrong nitric acid: The reſiduum from this ſolution evaporated to dryneſs, and then diſſolved in boiling water, did not produce the ſmalleſt effect on diſſolved gelatine.

The experiments on to No. 13. did not produce any tannin.

9. The reſiduum of a ſtrong infuſion of galls, treated as No. 8.

10. Iſinglaſs precipitated by infuſion of galls, diſſolved in ſtrong nitric acid, and examined as No. 4.

11. Twenty grains of ſumach diſſolved in half an ounce of ſtrong nitric acid, and treated as No. 8.

12. Twenty grains of Pegu cutch (which contains much mucilage) ſubjected to a ſimilar proceſs, by which much oxalic acid was obtained.

13. Twenty grains of catechu, called kaſcatti, treated ſimilarly, had, together with the four foregoing experiments, all the ſame reſults as No. 8, not any of them ſhewing any tannin.

14. Twenty grains of common catechu, diſſolved in ſtrong nitric acid, evaporated to dryneſs, diſſolved in water, and examined by iſinglaſs, depoſited a tenacious film inſoluble in boiling water, evidently compoſed of gelatine and tannin.

15. Twenty grains of oak bark treated in the ſame way, depoſited alſo an inſoluble film on the ſides and bottom of the veſſel.

16. Infuſions of galls, ſumach, and oak wood, of equal ſtrength, were mixed with nitric acid, in the proportion of half an ounce meaſure of each to one drachm of the acid, and did not then render iſinglaſs ſolution turbid.

But infuſions prepared from oak bark and the artificial tan, and managed in the ſame way, continued to precipitate the gelatine, until four drachms of the nitric acid had been added to each half ounce of the infuſion.

Theſe reſults ſhew that artificial tan is the moſt indeſtructible, but that the other tanning ſubſtances have conſiderable varieties in this reſpect. The tannin of oak bark reſiſts nitric acid longer than that of galls, ſumach, kaſcutti, or Pegu cutch. This laſt is replete with mucilage, and yields much oxalic acid, as before deſcribed: it ſeems alſo to be the moſt deſtructible of all the kinds of catechu: From theſe facts the author was induced to add the ſugar and gum to the artificial tan, to promote its deſtructibility; and expreſſes his belief that mucilage or gum renders the ſubſtances that contain it more deſtructible in the nitric acid, and in ſome caſes alſo prevents or impedes the formation of the tanning ſubſtance; which difference he thinks to be cauſed by the mucilage being in a ſtate of chemical combination in thoſe bodies.

§ III.

Experiments on the artificial tan.

A and B. When ſulphuric or muriatic acid was added to a ſolution of the new tan, it became turbid and depoſited a brown precipitate, which was ſoluble in boiling water, and was then capable of precipitating gelatine; in which particulars it reſembles the tannin of galls and other vegetable ſubſtances.

C. Carbonate of potaſh, added to a ſolution of the new tan, deepened the colour; the liquor became turbid, and depoſited a brown magma.

D. Five grains of dried artificial tan were diſſolved in half an ounce of ſtrong ammonia: the whole was then evaporated to dryneſs; and being diſſolved in water was found not to precipitate gelaten, unleſs a ſmall portion of muriatic acid was previouſly added.

E. Another portion diſſolved in ammonia was diſtilled: At firſt ammonia came over, and afterwards a yellow liquor, that had the odour of burned horn. The reſiduum was inſoluble in water, to which it only gave a ſlight yellow tinge.

On diſtillation it has an odour of burned horn, and yields ammonia.

F. The object of this experiment is to ſhew the ſtrange property of the new tan, of giving products analogous to animal matter (of which it yielded the odour in combuſtion on former trials), though prepared itſelf from vegetable ſubſtances. Some prepared from dry vegetable charcoal was diſtilled: Firſt a little water came over, then a little nitric acid, then a very ſmall portion of a yellowiſh liquor: The fire being then raiſed, the veſſels ſuddenly became filled with a white cloud, and ſo great a torrent of gas was almoſt exploſively produced as to overſet the jar: This gas, by its ſmell, appeared to be ammonia, and was formed into the cloud by the nitric acid vapour in the veſſels. The next jar of gas, which came ſlowly over, was carbonic acid, except a very ſmall part which ſeemed nitrogen gas. A bulky coal remained, that on incineration gave 1½ grains aſhes, which conſiſted principally of lime.

G. Fifty grains of this ſubſtance were diſſolved in four ounces of water and precipitated by iſinglaſs ſolution; eighty-one grains of which became thus combined with forty-ſix of the new tan. The remaining portion was not precipitated, and was therefore ſeparated on a filter and evaporated to dryneſs. It was a light brittle ſubſtance of a pale cinnamon colour, which, though compoſed of inodorous ſubſtances, had however a ſtrong ſmell itſelf of oak bark; which is remarked as a ſingular circumſtance; and this ſmell became ſtronger when the ſubſtance was put into water, in which it inſtantly diſſolved.

The ſolution was very bitter; acted but ſlightly on diſſolved iſinglaſs; produced a brown precipitate with ſulphate of iron, and with muriate of tin a black one; had no effect with nitrate of lime; but with acetite of lime gave a copious precipitate, of a pale brown colour. This ſubſtance appeared to be the tanning matter in the ſtate of extract.

§ IV.

Attempts to form tanning matter by oxi-muriatic acid unſucceſsful.

Several unſucceſsful attempts were made to form the tanning matter by oxi-muriatic acid. It therefore appeared, that though a variety of it could be produced by the action of ſulphuric acid on reſinous ſubſtances, yet nitric acid was the moſt effective agent.

Artificial tan, it is ſuſpected, might be formed from ſubſtances not charred.

The author ſuſpecting that the new tan might be formed from bodies not abſolutely converted into coal, and not being able to get any touch-wood, which he firſt thought of trying for this purpoſe, made the following experiment with indigo, which he knew to contain much carbon.

Experiments on indigo with this view.

One hundred grains of indigo, with one ounce of nitric acid diluted with a double quantity of water, was (when the efferveſcence had ſubſided), placed in a ſand-bath for ſeveral days till evaporated to dryneſs.

The reſiduum, of an orange colour, was in great part diſſolved by three ounces of diſtilled water poured on it, and gave a ſolution of a deep yellow, and intenſely bitter; which, with the ſulphate of iron, depoſited a ſlight pale-yellow precipitate, and with nitrate of lime, a ſmall white precipitate, having the character of oxalate of lime: With muriate of tin a copious white precipitate, that changed to a yellowiſh-brown; and with acetite of lead a beautiful deep lemon-coloured precipitate, which may probably prove uſeful as a pigment.

Ammonia rendered the colour much deeper, and with it depoſited a large quantity of fine yellow ſpiculated cryſtals, which did not precipitate lime from its ſolutions. Their flavour was very bitter.

It produces tanning matter.

Laſtly, when this ſolution was added to diſſolved iſinglaſs, it became turbid, and depoſited a tough elaſtic inſoluble film, and poſſeſſed the characters of gelaten combined with tanning matter.

Almoſt all vegetable bodies yield tanning matter, when ſubjected to repeated diſtillations with nitric acid.

By this experiment the poſſibility of producing tanning matter from bodies not converted into coal was fully aſcertained; and the author has ſince diſcovered that though indigo yields this matter more readily than moſt other vegetable bodies, yet almoſt all produce it when ſubjected to repeated diſtillations with nitric acid.

Reſin yields it by this treatment.

A. The common reſin did not produce the tanning ſubſtance with nitric acid, but by the aid of ſulphuric acid, as before related; yet upon this nitric acid being repeatedly abſtracted from it, its ſolution in water formed a tough yellow inſoluble precipitate with diſſolved gelaten, ſimilar to that by ſolution of indigo, and with other re-agents produced the following effects.

With ſulphate of iron, after 12 hours, it produced a ſlight yellow precipitate. With nitrate of lime no effect. With muriate of tin, after 12 hours, a pale brown precipitate. And with acetite of lead a very abundant precipitate of a yellowiſh white colour.

On repeating this experiment, the author remarked that during each diſtillation nitrous gas was produced, while the acid which came over was weakened, which made the cauſe of the change in the properties of the reſin evident. The following are the reſults of experiments tried with other reſinous ſubſtances.

As do likewiſe ſtick lac,

B. Stick lac, treated as deſcribed, copiouſly precipitated gelaten.

—and balſam of Peru,

C. Balſam of Peru during the proceſs afforded ſome benzoic acid, and gelaten was precipitated by the aqueous ſolution.

—and benzoin,

Benzoin, after the ſublimation of ſome benzoic acid, yielded a reſiduum, which yielded with water a pale yellow ſolution, of a very bitter flavour.

This ſolution with ſulphate of iron produced a ſlight pale yellow precipitate. With nitrate of lime, no effect. With muriate of tin in ſolution, a ſmall quantity of browniſh white precipitate. With acetite of lead, a copious pale yellow precipitate. And with ſolution of iſinglaſs, a denſe pale yellow inſoluble precipitate.

E. Balſam of Tolu afforded benzoic acid, and the ſolution of the reſiduum precipitated that of gelaten.

—and dragon blood,

F. One hundred grains of dragon's blood in powder mixed with one ounce of nitric acid, evolved much gas; an ounce of water was then added; and the digeſtion in a ſand bath being continued, after it produced chaſiccation on the dry yellow maſs that remained, a brilliant feather-like ſublimate aroſe, which weighed rather more than ſix grains and had the aſpect, odour, and properties of benzoic acid.

The reſiduum, of a brown colour, formed with water a gold coloured ſolution, which was not affected by nitrate of lime: But with ſulphate of iron, and with muriate of tin it formed browniſh yellow precipitates; and with acetite of lead one of a lemon colour.

Gold was precipitated by it in the metallic ſtate, and the containing glaſs coloured purple; and with diſſolved iſinglaſs it produced a deep yellow inſoluble depoſit.

As dragon's blood ſimply expoſed to heat did not produce any benzoic acid, the author is inclined to believe, that in the firſt experiment this acid was obtained as a product, and not as an educt.

—and gum ammonia,

G. Gum ammonia gave a browniſh yellow, bitter aſtringent ſolution; which with ſulphate of iron became of a darker colour, but produced no precipitate.

With nitrate of lime, a ſlight precipitate. With muriate of tin and acetite of lead, copious yellow precipitates; and with gelaten a bright yellow inſoluble depoſit.

—and aſſa fœtida.

H. Aſſa fœtida yielded a ſolution which precipitated gelaten in a ſimilar manner to that deſcribed.

Solutions of elemi, tacamahac, &c. did not affect gela

I. Solutions of elemi, tacamahac, olibanum, ſandarach, copaiba, maſtich, myrrh, gambauge, and caoutchouc, although

though they precipitated the metallic ſolutions, did not affect gelaten; but poſſibly might have done ſo, if the proceſs had been more frequently repeated.

—not that of ſarcocol,

K. Sarcocol alſo produced ſimilar reſults.

—nor of gum Arabic,

L. Gum Arabic afforded oxalic acid but no tan.

—nor of tragacanth,

M. Tragacanth yielded much of ſaclatic acid, of oxalic, and of malic acid, but not the leaſt tan.

—nor of manna.

N. Manna gave oxalic acid, part of which ſublimed in the neck of the veſſel.

Its reſiduum formed a brown ſolution, which produced precipitates of the following colours: With ſulphate of iron, a pale yellow; with muriate of tin, a pale brown; with acetite of lead, a browniſh white. From nitrate of lime, oxalate of lime was copiouſly precipitated by it; but with iſinglaſs ſolution no effect was produced.

Liquorice ſolution precipitates gelaten.

O. Nitric ſolution of liquorice yielded precipitates with ſulphate of iron and muriate of tin, after twelve hours, ſlight brown. With acetite of lead, a browniſh red. With nitrate of lead, a brown. And with gelaten, one of a yellowiſh brown, inſoluble, and ſimilar to other precipitates from it by tan.

Guiacum ſolution gave a ſlight precipitate with gelaten, which was ſoluble in water.

P. On guiacum nitric acid acted with great vehemence and ſpeedily diſſolved it: The reſiduum was almoſt totally ſoluble in water; and this ſolution produced effects on the metallic ſalts ſimilar to thoſe recited; but with gelaten formed a ſlight precipitate, which was ſpeedily diſſolved by boiling water. The remainder of the ſolution evaporated gave a large quantity of cryſtalized oxalic acid; ſo that in this reſpect guiacum was ſimilar to the gums, and unlike the reſins.

§ V.

Experiments on ſeveral roaſted vegetable ſubſtances, which do not affect gelaten.

As many vegetable ſubſtances when roaſted yield a liquor by decoction, reſembling ſolution of artificial tan, the author tried thoſe ſimilarly prepared, of dried peas, horſe beans, barley, and wheat flour, none of which gave any precipitate with gelaten.

Coffee gives a precipitate with it, which is again ſoluble in water.

The decoction of coffee alſo gave no precipitate till after ſeveral hours, and then one ſoluble in boiling water; but this might be occaſioned, the author thinks, from want of ſome particular nicety which may be required in roaſting ſuch bodies ſo as to make them yield tan; which opinion was corroborated by experiments made by decoction of chicoree (probably endive) root, prepared in the ſame manner, which produced a precipitate with gelaten after ſome time, though not at firſt, which was apparently diſſolved in boiling water, but depoſited again in its original ſtate, on cooling. The author therefore is inclined to believe that the tanning ſubſtance is really developed in many vegetable matters by heat alone; but that a certain degree of heat, not eaſy to determine is abſolutely neceſſary for this effect.

Nitric acid added to theſe decoctions gives tanning properties to their reſidue.

A ſmall quantity of nitric acid added to any of the decoctions juſt mentioned, and evaporated to dryneſs, produced a reſiduum, having all the properties of the tan produced from coal.

§ VI.

The production of a variety of the tanning ſubſtance before mentioned, by the action of ſulphuric acid on the reſins, amber, &c. ſuggeſted the following experiments on camphor; the reſults of which tend to increaſe the knowledge of its properties.

Experiments on Camphor with Sulphuric Acid.

The only facts hitherto related relative to the effects of ſulphuric acid on camphor, are that a brown or reddiſh brown ſolution is formed, from which water precipitates the camphor unchanged; but this only happens at a certain period of the operation; for if it be longer continued, the following effects will be produced.

Experiments with ſulphuric acid on camphor.

A. One ounce of concentrated ſulphuric acid was added to one hundred grains of camphor, which diſſolved gradually, after firſt becoming yellow; in about an hour, the liquor having progreſſively changed to reddiſh brown, brown, and at laſt blackiſh brown, much ſulphureous acid gas was produced, and continued to encreaſe during four hours, when the whole appeared a thick black liquid, having no other odour but that of ſulphureous acid; after two days the production of the gas was much diminiſhed; the containing alembic was then put in a ſand bath, moderately hot, by which more ſulphureous gas was obtained; but this ſoon abated; at the end of two days more, ſix ounces of water was gradually added, by which the liquor changed to a reddiſh-brown, a coagulum of the ſame colour ſubſided, the odour of ſulphureous acid gas was immediately annulled, and was ſucceeded by one which much reſembled a mixture of oils of lavender and peppermint.

An odour yielded by it like oils of lavender and peppermint.

The whole was then diſtilled gradually; when the water came over impregnated with the odour laſt mentioned, accompanied by a yellowiſh oil, which floated on the top, and was computed to amount to three grains.

B. When the whole of the water had come over, there was again a ſlight production of the ſulphureous acid gas; two ounces of water were then added, and the diſtillation continued (without the recurrence of the former odour) till a dry blackiſh brown maſs remained; this maſs was well waſhed with warm diſtilled water, by which nothing was extracted; but two ounces of alcohol digeſted on it for 24 hours formed a very dark brown tincture.

The reſiduum was digeſted with two ounces more alcohol, and the proceſs repeated till the alcohol ceaſed to act.

The reſiduum had now the appearance of a compact ſort of coal in ſmall fragments, which were well dried, and after being expoſed to a low heat in a cloſe veſſel, weighed fifty-three grains.

C. From different portions of the alcohol ſolution, added together and diſtilled in a water bath, a blackiſh brown ſubſtance was obtained, which had the appearance of a reſin or gum with a ſlight odour of caromel, and weighed 49 grains.

The products obtained from the 100 grains of camphor treated with ſulphuric acid, were,—

Products from camphor and ſulphuric acid.

	Grains.
A. An eſſential oil, having ſomewhat of an odour of a mixture of lavender and peppermint, about	3
B. A compact and very hard ſort of coal, in ſmall fragments, - - - - -	53
C. A blackiſh-brown ſubſtance, of a reſinous appearance, - - - - -	49
	105

The increaſe of weight of five grains is attributed partly to water retained by the laſt ſubſtance, and partly to oxigen united to the carbon.

The ſubſtance C had the following properties:

1. It was bitter and aſtringent, had the odour of caromel, and formed with water a dark-brown ſolution.

2. This ſolution produced very dark-brown precipitates with ſulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was precipitated by it in the metallic ſtate from its ſolution.

4. By ſolution of iſinglaſs the whole was precipitated; ſo that after four hours a colourleſs water only remained.

Subſtance C (from camphor) precipitates gelaten.

This precipitate was nearly black, and was inſoluble in boiling water: from whence, and its effects on ſkin, it was evidently a variety of tanning matter much reſembling that obtained from reſinous bodies by ſulphuric acid.

But this ſort of tan had leſs effect on ſkin than that procured from carbonaceous ſubſtances by nitric acid, and its precipitate from gelaten was more flocculent and leſs tenacious.

However, when a ſmall quantity of nitric acid was added to the ſolution of the ſubſtance obtained from camphor, and when the reſiduum, after evaporation to dryneſs, was diſſolved in water, a reddiſh-brown liquor was formed, which acted in every reſpect ſimilar to the tanning ſubſtance obtained from the varieties of coal by the nitric acid.

§ VII.

From the experiments related, it appears that three varieties of the tanning ſubſtance may be formed.

The three varieties of artificial tan.

1ſt. That produced by nitric acid with any carbonaceous ſubſtance, whether vegetable, animal, or mineral.

2d. That formed by diſtilling nitric acid from common reſin, indigo, dragon's blood, and various other ſubſtances.

3d. That which common reſin, elemi, aſſafœtida, camphor, &c. yield to alcohol, after they have been previouſly digeſted with ſulphuric acid.

On theſe products the author makes the following remarks:

Remarks on them.

The firſt variety is the moſt eaſily formed. From ſome experiments made purpoſely it appears, that, after making allowance for a ſmall quantity of moiſture and of nitric acid remaining, 100 grains of vegetable charcoal yield 116 of the dry tanning ſubſtance.

100 grains charcoal yield 116 tanning matter.

Carbon the baſe of the tanning matter.

From the manner in which it is produced, carbon is evidently the baſe and predominating eſſential ingredient in this ſubſtance.

It alſo contains oxigen, hidrogen, and nitrogen.

From § III. experiment F, it alſo appears that the other component parts are oxigen, hidrogen, and nitrogen; for when the artificial tan was diſtilled, ammonia and carbonic acid were obtained, excluſive of a ſmall portion of a yellow liquor that appeared to be of an oily nature, from being inſoluble in water and alcohol.

It has an odour like animal matter when burned, and one of oak bark when precipitated as in G, § III.

Many of the properties of the tanning ſubſtance prepared from coal by nitric acid are very remarkable, particularly thoſe noticed in § III. experiment F; of its having the odour of animal ſubſtances when burned, though prepared from vegetable matter; and in experiment G, of the precipitate having the odour of oak bark, though the component materials were inodorous.

It reſembles vegetable tannin in moſt properties.

But its moſt extraordinary properties are thoſe in which it ſo nearly approaches the vegetable *tannin*, which it perfectly reſembles in its ſolubility in water and in alcohol, in its action on gelaten and on ſkin, in its effects on the metallic ſolutions, on the alkalis, and on the earths.

Difference between it and tannin.

The ſulphuric and muriatic acids alſo affect its ſolutions, as they do thoſe of tannin; and the only marked difference between artificial tan and tannin is, that the former is produced by nitric acid, while the varieties of the latter are more or leſs deſtroyed by it; but here it muſt be remembered, that even the varieties of tannin do not accord in the degree of deſtructibility.

Second variety of artificial tan.

The ſecond ſpecies of the tanning ſubſtance is obtained from a variety of vegetable bodies before recited, by digeſting and diſtilling them with nitric acid. It is therefore not ſo readily prepared, and the quantity of it produced is leſs in proportion to the ſubſtance from which it is prepared.

Theory of its formation.

As reſin and ſome other bodies do not afford it until they have been repeatedly treated with nitric acid, and as, during each operation, nitrous gas is produced, while the ſtrength of the acid which comes over is diminiſhed, the author thinks it almoſt certain that the tanning ſubſtance is formed in conſequence of part of the oxigen of the nitric acid becoming combined with the hidrogen of the original body, ſo as to form water; and the carbon being thus in ſome meaſure denuded, is rendered capable of being acted on gradually by the nitric acid, in a manner nearly ſimilar to what takes place when it has been previouſly converted into coal.

The precipitates of this tanning ſubſtance from gelaten are always pale or deep yellow, while thoſe formed by the firſt ſpecies are conſtantly brown; which induces the author to believe that the different colours of the precipitates depend on the ſtate of the carbon of the tannin.

The quantity of artificial tan obtained from reſin and other bodies, was always leſs than that from coal, or even from the ſame bodies previouſly converted to coal in the humid way by ſulphuric acid. The cauſe of this ſeems to be, that a number of other products are ſimultaneouſly formed with the tanning ſubſtance, all of which require more or leſs carbon as an ingredient; ſo that, according to the affinities which prevail, ſome bodies afford but little, and others none of it.

Quantity of artificial tan leſs from reſins than from coal, accounted for.

The greateſt proportion of this ſubſtance was yielded by indigo, common reſin, and ſtick lac.

Its proportions from different ſubſtances.

The quantity obtained from aſſafœtida and gum ammoniac was leſs.

Benzoin, balſam of Tolu, balſam of Peru, and dragon's blood, were inferior to the former in this reſpect; ſo that the production of benzoic acid ſeemed to counteract the formation of the tanning ſubſtance. But oxalic acid, when formed in any conſiderable quantity, ſeemed abſolutely to prevent the formation of this ſubſtance: for gum arabic, tragaçanth, manna, and guiacum, which produced oxalic acid in abundance, yielded no tanning matter.

Benzoic acid when formed counteracts the formation of the tan; and oxalic acid, when produced, prevents it entirely.

Common liquorice ſeems to be an exception; but the author ſuppoſes that the ſmall quantity of tan produced by it, was formed by the action of the nitric acid on a portion of uncombined carbon, which being in a ſtate approaching to coal, is probably the cauſe of the blackneſs of the common liquorice.

The third variety of the tanning ſubſtance appears to be uniformly produced during a certain period of the proceſs; but by a long continuance of the digeſtion there is reaſon to think it is deſtroyed.

Third variety of artificial tan.

Subſtances, ſuch as gums, which yield much oxalic acid, do not apparently afford any of this tanning matter.

The energy of its action on gelaten and ſkin is certainly inferior to that of the firſt variety, into which however it may eaſily be converted by nitric acid.

From the mode of its formation, there does not appear to be any evidence of its containing nitrogen like the firſt and ſecond varieties, and perhaps the abſence of nitrogen may be the cauſe of its leſs powerful action.

Experiments of Meſſrs. Biggin, Prouſt, and Davy on tannin, noticed.

In the courſe of the communications on this ſubject, Mr. Hatchett notices the experiments on tannin by Mr. Biggin, the great contributions of M. Prouſt to the elucidation of its nature and properties, and the very great extenſion of, and valuable additions to the ſame, from the ingenious labours of Mr. Davy, particularly his diſcovery of the ſingular fact that terra japonica, or catechu, conſiſts principally of tannin.

Medicine, arts, &c. may derive great benefit from farther inveſtigations of gums, reſins, &c.

The author alſo greatly recommends the farther inveſtigation of the nature of the gums, reſins, balſams, and gum-reſins, by every poſſible method; and is of opinion, that medicine, arts, and manufactures may derive many advantages from it, and the myſterious proceſſes of vegetation probably receive conſiderable elucidation.

VI.

On carboniſed Turf. From a Report made to the Prefect of Police (at Paris) on the Methods employed for reducing it to this State. By MM. CALLIAS *and Co*[*].

The uſe of turf very ancient. It produces no deleterious effects.

THE uſe of turf for domeſtic fuel is of a very ancient date: Some of the moſt eminent men of ſcience have pronounced that it does not produce any deleterious effects. Without citing the examples of England (Ireland), Scotland, and Holland, where great quantities of it are conſumed, we will confine ourſelves to the uſe made of it in France, in the *(ci-devant)* provinces of Flanders, Artois, and Picardy.

Its uſe is now tolerated in Paris to relieve the ſcarcity of wood: the lime-burners, plaſter-bakers, brick-makers, and waſhers, make great uſe of it both in the city and its vicinity; and it has never been perceived that thoſe who lived within the influence of its ſmoke, have experienced any bad effects from it. The commiſſioners (employed to make this report) obſerve, that the great volume of ſmoke which is diſengaged on the commencement of its combuſtion, is only cauſed by a great portion of water contained by the turf, which is expanded into ſteam by the heat; ſoon afterwards this ſmoke is combined with an acid analogous to that of vegetable ſubſtances, which, far from making the air deleterious, tends on the contrary to neutralize the vapours of infection which it may contain. It is true that ſometimes, for an inſtant, the turf in combuſtion exhaled an odour of empyreumatic oil, in the form of gaſeous vapours, but this odour is by no means injurious to the animal organiſation, but, on the contrary, is beneficial in nervous affections.

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[*] Sonini's Journal, Tom. II. p. 324.

Its ſmoke is cauſed by contained water; with an acid vapour, which removes infection.

But if this odour is diſagreeable when the turf is burned in towns, villages, and private houſes, this complaint cannot take place when it is burned in the open air at a diſtance from all habitations, which will be effected by its previous carboniſation, as managed by MM. Callias and Co. therefore the company merit the public protection.

Charing prevents all the unpleaſant effects of its odour.

In 1785 the French Government took a great intereſt in what related to the carboniſation of turf, and granted 80,000 francs to a company to erect a furnace for this purpoſe on the ground of the Capuchins. The method of this company was that of extinguiſhing, but their plan did not ſucceed, and the works were abandoned.

Company at Paris for charing turf, aſſiſted by the government, did not ſucceed.

A new company tried, ſome time after, the ſame enterprize, at its own expence: the method of operating in cloſed veſſels was propoſed: the experiments made were on a great ſcale, and were attended with a ſucceſs that was certified by the commiſſioners of government: a memoir printed in 1790, by M. Morclot, contained theſe facts, with a ſtatement of the ſuperiority of turf-charcoal over that of wood. But the diſaſtrous events of the revolution put an unhappy end to this enterprize which promiſed ſo well.

Another company—promiſed well—but the revolution cauſed its failure.

At preſent MM. Callias and Co. offer to the public an additional ſpecies of fuel to that hitherto in uſe, a charcoal of a new proceſs, the materials of which are ſpread with profuſion over the territory of France, and the conſumption of which, being ſubſtituted for that of wood, will at the ſame time be an object of economy to individuals, and of incalculable ad-

Callias and Co. charr turf by a new proceſs.

The uſe of turf charcoal. It will be cheap,

vantage

and prevent the deſtruction of the foreſts.

vantage to the management of the foreſts. Timber for the conſtruction of houſes and furniture, and timber for ſhip-building, daily increaſe in price, becauſe they become more ſcarce. Some of the foreſts have become reduced, as the freſh growths in them do not keep pace with the deſtructive inſtrument that overturns them; ſome of them are entirely deſtroyed, and the ground converted into ornamental gardens; and thus each year, each month, each day, conducts us inſenſibly to a moſt alarming dearth of timber. Already the price of fire-wood is tripled, and Paris is on the eve of being deprived of a combuſtible which, as yet, has not been replaced to advantage.

The carbonization uſed by Callias is very perfect.

The commiſſioners compliment MM. Callias and Co. whoſe method of carboniſation is peculiar to themſelves, and calculated conformably to the laws of combuſtion in its two firſt ſtages; that is to ſay, before the arrival of its third degree, or that of abſolute combuſtion. MM. Callias and Co. by their method, direct the carboniſation at their pleaſure and in an invariable manner; they are always ſure of obtaining a perfect charcoal, without ſmoking-pieces, and without any riſk of forming it into a pyrophorus, which ſometimes happens in the carboniſation performed in cloſed veſſels. Their manner of proceeding is alſo very economical; and what proves that they work with intelligence is, that they daily improve, and already are able to ſave ten hours out of 48 in each carboniſation.

Experiments made with Charcoal of Turf.

Turf-charcoal yields more heat than wood-charcoal.

1ſt. The charcoal of turf kindles a little ſlower than that of wood, but when it is once in compleat ignition, it throws out much more heat; its flame is alſo more elevated, and it yields no odour, except a very ſlight one of ſulphur, which ceaſes when it is fully lighted.

Cauſes water to boil four times as ſpeedily.

2. Charcoal of turf, in equal quantity with charcoal of wood, cauſed a given quantity of water to boil four times; while that of wood cauſed it only to boil once. The firſt is then ſuperior to the ſecond in a quadruple proportion.

3. To prove that turf charcoal emits more heat than wood charcoal, the following experiment was made.

It fuſed 11 oz. of gold in eight minutes; wood-charcoal did the ſame in ſixte

With turf charcoal, in a goldſmith's furnace, eleven ounces of gold were fuſed in eight minutes, which with wood charcoal was not performed in leſs than ſixteen minutes. The gold loſt nothing of its malleability in the fuſion with the turf; but, on the contrary, it was neceſſary to add ſome reductive flux, to that fuſed by the wood charcoal, in order to reſtore the malleability which it had loſt.

minutes. The malleability of the gold was preſerved.

4. Iron made red-hot by charcoal of turf in a forge, became more malleable; which proved that it gave none of its carbon to the metals with which it came in contact.

5. Finally, Turf charcoal laſts longer in a ſtate of ignition than charcoal of wood, and its heat is conſtantly equal.

Iron heated by it becomes more malleable. It laſts longer than wood-charcoal.

Concluſion.

1. The odour of turf in combuſtion is noways deleterious. This truth has been confirmed by the moſt diſtinguiſhed chemiſts; and is beſides proved by the conſtant uſe made of this fuel in the *ci-devant* provinces of Flanders, Artois, and Picardy.

Its odour not at all unwholeſome.

2. It is deſirable that the carboniſation of turf may be encouraged, on account of the great advantages which may reſult from the uſe of this new ſpecies of charcoal, both for private conſumption and for large works.

Its uſe ought to be encouraged,

But the greateſt matter in its favour is, that its uſe tends to diminiſh the felling of the foreſts, whoſe extenſion ought to be promoted by every means poſſible, and which nothing tends ſo much to deſtroy as the uſe of wood charcoal.

and will preſerve the woods.

VII.

Account of the Cataracts and Canal of Troellhætta, in Sweden, (from a Work relative to them by Colonel SKIOELDEBRAND. *Publiſhed in one Volume Quarto, at Stockholm.)*

THE cataracts of Troellhætta produce one of the fineſt effects which nature affords in Europe. The river of Gothie is the only outlet of the vaſt lake of Wener, navigable through its whole extent. This river, which falls in the North ſea near Gothemberg, as ſoon as it departs from the lake, which is much more elevated than the ſea, rolls its waters with impetuoſity, and daſhes them againſt ſteep rocks, whoſe reſiſtance forms a ſucceſſion of cataracts, which without being individually very high, form altogether a moſt ſtriking object. The imagination is the more affected by this ſight, as the ſurrounding ſcenes are of a dark and melancholy character, conſiſting of grey rocks,

The cataracts are formed by the river Gothie ſoon after its departure from lake Wener.

They form a very ſtriking object.

crowned

Ruins of locks which had been formerly conſtructed in the bed of the cataracts.

crowned with ancient firs, and of frightful precipices, formed by the burſting of the locks and banks, which the fury of the water has overturned. Theſe laſt were conſtructed in the bed of the cataracts, in order to render the river navigable through its whole length; but this daring work of man could not reſiſt the reſiterated efforts of nature, and therefore it was neceſſary to have recourſe to another plan.

The canal paſſes by the ſide of the cataracts. Is partly cut in the rock. Its breadth, depth, number of locks, time of excavation, and coſt. The extent of its navigation, and number of veſſels which have paſſed.

The canal newly conſtructed paſſes by the ſide of the cataracts, and its bed is partly formed in the natural rock, and partly in a marſhy ſoil. It was began in 1794, and finiſhed at the end of ſix years, in 1800. Its breadth is 22 feet, and its depth ſix feet and an half. Its locks are eight in number, and its coſt amounted to the ſum of 59833 pounds ſterling, which was collected by ſubſcription. By means of this canal there is a continued navigation, without any interruption, from the province of Wermeland to Gothemberg. In 1802 the number of veſſels which had paſſed this canal amounted to 1380, which is at the rate of 1190 each year.

VIII.

Letter from H. B. K. *on the Production of Nitrous Acid, and other Facts*[*].

To Mr. NICHOLSON,

SIR,

Experiments announced. Carbonate of potaſh in water was galvanized, and emitted carbonic gas.

AS Mr. Accum has not anſwered my paper, he therefore knows of no experiments which ſhew the formation, of the nitrous acid; but anxiouſly impreſſed with the ſubject, I have been performing ſome experiments, which I think will throw great light upon the cauſe of the nitrous acid appearing in electrical experiments.

The potaſh became capable of deflagration like nitre; and ſolution of ſilver ſhewed the preſence of muriatic acid.

I paſſed the galvanic fluid through a watery ſolution of the carbonate of potaſh, made by diſtilled water, confined in a glaſs tube where no atmoſpherical air could have acceſs to it, and I found a great production of air come from the ſolution, which upon examination was pure carbonated air; and then examining the ſolution by dipping a piece of paper into it; upon its being dipped the paper ſhewed evident ſigns of nitre upon it, and when burned it detonated the ſame as nitre would have done; and alſo with the ſolution of ſilver the alkaline ſolution gave ſome faint indications of the marine acid being preſent in it. That the ſolution ſhould give indications of poſſeſſing both the nitrous and marine acids is not ſo ſurprizing; as we have the ſame products in firing oxigen and hidrogen gaſes, according to the foreign experiments, principally the nitrous acid, but with it a ſmall quantity of the marine acid.

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[*] See our Journal, X. 105, 214, and XI. 105.

Remark. Theſe two acids are found on the common detonation of oxigen and hidrogen.

I then filled the tube (after waſhing it clean) with pure diſtilled water, and ſent through it the galvanic fluid; and I obſerved a generation of airs, which, upon examination, appeared to be the airs uſually formed in theſe experiments, as they exploded.

Pure water galvanized gave theſe gaſes.

After this I filled the tube with a ſolution of pure potaſh in diſtilled water, and no air came from it upon galvaniſing it; if any, it was carbonated air: But upon examining the ſolution, it gave clearly the ſame indications of poſſeſſing the nitrous and marine acids, as the carbonated ſolution of potaſh did in the firſt experiment.

Pure potaſh and water gave no gas by galvanizing, but indicated the two acids of the firſt experiment.

Now exempt from all hypotheſes, let us examine theſe intereſting facts: The carbonated potaſh had its carbonic acid air expelled, clearly from the acid or acids; as we know that it could not part with its carbonic acid air, but from the action of a ſtronger acid. Alſo another more eſſential fact it proves to us, that the generation of the peculiar airs, what are called oxigen and hidrogen gaſes, are owing to the acids; for when the potaſh was in the water as to arreſt or attract them, there were neither of theſe airs produced; and upon examining the diſtilled water (in the experiment in which they were produced) after their production, there was no acid in the water, but it was pure diſtilled water. Therefore, beyond a doubt, the nitrous acid is eſſentially concerned in the production of theſe peculiar oxigen and hidrogen gaſes: indeed Mr. Cruickſhanks ſays, that upon fuſing theſe airs, he found in the reſiduum the nitrous acid.

Remarks. The carbonic acid was expelled from potaſh by a ſtronger acid produced. Whence it is inferred that the oxigen and hidrogen aroſe from theſe acids.

Theſe experiments were performed by two ſhort gold wires attached to each end of the galvanic pile, But upon placing a pretty long iron wire to the ſilver end of the pile inſtead of the

The conducting wires were gold.

the gold one, a little hydrogen gas was produced; even when the potaſh was mixed with the diſtilled water, though there was none when it was a gold wire.

I hope Mr. Nicholſon you will not refuſe the inſertion of theſe intereſting facts in your Journal, for I have made the relation of them as brief as poſſible that they might not occupy too much room *.

H. B. K.

London, Auguſt 15.

IX.

Report of M. Debuc's *Memoir on Acetic Acid, made by M. M.* Planche *and* Boullay, *by Order of the Society of Pharmacy at Paris* †.

M. Debuc repeated M. Badollier's proceſs to obtain acetic acid from acetate of lead by ſulphate of copper.

. Debuc ſaw in the *Annales de Chimie*, No. 109, a method of M. Badollier, apothecary at Chartres, for obtaining acetic acid very readily from a mixture of equal parts of ſulphate of copper and acetate of lead by a moderate heat.

The product uſed in manufacture produces an effect contrary to the common acetic acid.

Relying on this proceſs M. Debuc made an exact mixture of two pounds of ſulphate of copper and an equal quantity of acetate of lead, which he expoſed in a diſtilling apparatus on a ſand bath to a moderate fire, which he increaſed by degrees during the operation, which laſted for ſix hours: the product obtained was 26 ounces. It was given to a manufacturer, without examination, and being uſed in his buſineſs produced an effect entirely contrary to the acid extracted from cryſtals of copper.

The proceſs again repeated, and its reſults carefully examined.

This circumſtance determined M. Debuc to repeat the proceſs as before, and examine its reſults carefully: In which he obſerved the mixture of the two ſalts to become paſtey, which is eaſily explained by the difference of the concentration of the acid in the ſulphate of copper, from that in the ſulphate of lead. The products of this experiment were,

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* I am extremely ſorry that this communication was by miſtake placed among papers already printed; which alone has cauſed the delay in its appearance.—W. N.

† Annales de Chimie, Tom. LIV. p. 145.

Products of this proceſs.

1. Four ounces of water ſlightly acidulated.

2. Four ounces of a liquor more acid than the firſt, and which M. Debuc compares to good vinegar of *Saumur*.

3. Eighteen ounces of a very limpid liquor, with a lively and penetrating odour of acetic acid mixed with ſulphurous acid.

The reſidue is in layers of different colours.

The reſidue, weighing 38 ounces, appeared to M. Debuc in different layers more or leſs red, according to their diſtance from the bottom of the retort; and he found the upper part covered with a whiteiſh powder, ſlightly inclined to a citron colour, in which he recogniſed the preſence of ſulphur.

Precipitates formed with the third product by different ſalts.

Barytes, the muriate of lime, and the acetate of lead formed immediately conſiderable precipitates with the third product.

Sulphurous acid being produced, induces M. Debuc to ſuppoſe that acetic acid is ſuper-oxigenated vinegar.

M. Debuc obſerves that the decompoſition of the acetate of lead by the ſulphate of copper may be eaſily explained; but that here there is a production of ſulphurous acid, and a decompoſition of the ſulphuric acid from the abſorption of its oxigen by the vinegar; which is a ſingular phenomenon, that has no agreement with the affinities of the acidifying principle for the *acidifiable* and *ſalifiable* baſes; he leaves the explanation of this matter to more experienced chemiſts, and only notices that the tranſportation of the oxigen of the ſulphuric acid to another baſe, ſuggeſts the idea, *that acetic acid is ſuperoxigenated vinegar*.

M. Debuc's method of freeing the third product from ſulphurous and ſulphuric acids.

Acetic acid produced, one degree weaker than the common kind.

M. Debuc ſucceeded in freeing his third product from the ſulphurous and ſulphuric acids, by letting it remain for about 24 hours, on twelve grains of ſalt of tartar, and about two ounces of black oxide of manganeſe pounded fine, and after that diſtilling it ſlowly; by this rectification he obtained a pound of pure acetic acid of a lively and agreeable odour, and of about 10 degrees ſpecific gravity; which is one degree leſs than that of radical vinegar well rectified, obtained from acetate of copper.

The author concludes from this,

1. That the product of two pounds of acetate of lead, treated with an equal quantity of ſulphate of copper, is twenty-ſix ounces; of which four ounces is acidulated water, an equal portion ſtrong vinegar, and eighteen ounces acetic acid altered by the ſulphurous and ſulphuric acids.

M. Debuc concludes that the acetic acid produced only dif-

2. That the eighteen ounces, forming the third product, rectified as recited, does not differ from that drawn from cryſtals of acetate of copper, but by its leſs denſity.

3. That

fers in ſtrength from the common kind, and may be ſubſtituted for oximuriatic acid, in ſome caſes to advantage.

3. That in many caſes this acid may be ſubſtituted for oximuriatic acid, as an object of ſalubriety without poſſeſſing its inconveniencies.

The reporters repeat M. Badollier's proceſs, with M. Debuc's modifications.

The reporters repeated the proceſs of M. Badollier with the modifications adviſed by M. Debuc as follows.

They introduced a mixture of two pounds of ſulphate of copper, and the ſame quantity of acetate of lead into a glaſs retort, placed it on a ſand bath, and adjuſted to it a tubulated receiver, which communicated with two bottles of a Wolf's apparatus; the firſt of which contained diſtilled water, and the ſecond many pounds of lime water; from this laſt a tube was paſſed underneath a jar in an hydro-pneumatic apparatus; the retort was heated gradually to the end of the operation, which laſted more than 10 hours; and the following products were drawn from the receiver.

Products of their proceſs from the receiver.

1. Eight ounces of a liquor ſimilar to diſtilled vinegar, but with a leſs agreeable odour.

2. Ten ounces of a liquor with an unpleaſing odour of acetic acid, more penetrating than the firſt, and not containing any trace of ſulphurous or ſulphuric acids.

3. Finally ſeven ounces of a liquor of great limpidity, with a very pungent odour of ſulphurous acetic acid, and which did not precipitate muriate of barytes.

A conſiderable diſengagement of an elaſtic fluid was obſerved, which became perceptible as ſoon as the retort began to run, and which laſted during the whole operation.

Carbonic acid gas evolved.

This gaſeous fluid was abſorbed almoſt totally by the lime water, forming with it a very abundant white precipitate; which, gathered on a filter, and dried, proved to be carbonate of lime: It weighed two hundred and fifty grains, which made the carbonic acid equal, according to the known proportions of this ſubſtance, to eighty-five grains; atmoſpheric acid alone paſſed under the jar mixed with ſome carbonic acid gas: no trace was perceived of hydrogen gas.

Many layers of different colours were found in the retort.

The firſt was of a beautiful green, ſurrounded with a circle of yellowiſh white towards the ſides.

The ſecond, much more thick, was of a red colour, greatly like copper in very ſmall particles.

The third was a mixture of ſulphate of lead and of copper apparently in the metallic ſtate.

The laſt layer, which occupied the bottom, of a black colour, and ſhining, was a mixture of ſulphate of lead and of charcoal.

The loweſt layer of the reſidue is a mixture of ſulphate of lead and charcoal.

The ſame experiment with the ſame quantities of the ſalts, was repeated a ſecond time, with the precaution of reducing the ſulphate of copper by diſiccation to $\frac{11}{16}$ of its weight. The product from this was preferable to the other.

The ſame proceſs again repeated; drying the ſulphate of copper, the produce is better.

The ſecond and third products were mixed and rectified on carbonate of potaſh and oxide of manganeſe, with the precautions indicated by M. Debuc: This rectification produced an acetic acid of nearly the ſame ſpecific gravity as that afforded by ſimple diſtillation from cryſtals of copper, but of a leſs ſtrong odour, leſs agreeable, and beſides mingled with ſulphurous acid.

Second and third products rectified.

A weaker and leſs agreeable acetic acid produced, mingled with ſulphurous acid.

The reporters think that M. Debuc is deceived in his theory, "*that acetic acid is vinegar ſuper-oxigenated by the oxigen of the ſulphuric acid paſſing to the vegetable acid,*" for he has not conſidered,

1. That the acetic acid is almoſt all obtained, before the ſulphurous acid becomes perceptible.
2. That the metallic oxides, which are the baſis of the ſalts employed, have leſs attraction than ſulphur to oxigen.
3. That the diſengagement of the carbonic acid is much more likely to explain the matter.

Reaſons why M. Debuc's theory is erroneous.

The conſiderable production of carbonic acid, and the preſence of charcoal in the reſidue, ſurpriſed the reporters the more; as *MM. Boddolier and Darac* (the firſt in his notice of the preparation of acetic acid; the other in a memoir, in other reſpects very intereſting, on the difference of acetous and acetic acids,) poſitively aſſert that in the operation related, *there was no other gaſeous production but that of part of the air contained in the veſſels, eſpecially no carbonic acid, and not an atom of charcoal in the reſidue.*

The production of carbonic acid and charcoal from the proceſs, is contrary to the aſſertions of MM. Bodollier and Darac.

The reſult found by the reporters ſo different from that of *M. Darac,* in an experiment on which he ſupports his theory of the identity of the acetous and acetic acids, was ſo favourable to the theory of *M. Chaptal,* that they would have been induced to decide in favour of the opinion of the latter, if the following comparative experiments had not confirmed them in a contrary notion, and appeared to them one of thoſe, of which M. Darac might moſt avail himſelf.

An experiment made, favourable to M. Darac's opinion.

To four ounces of pure concentrated radical vinegar (extracted from cryſtals of copper by heat alone) were added by degrees four ounces of ſemi-vitreous oxide of lead (litharge) in powder; which compleatly diſſolved in it by heat, there even remained an exceſs of acid, perceptible in the ſtrong odour of the ſolution. Being laid by to cool, it produced a very irregular cryſtalline maſs.

Four ounces of this maſs of acetic lead, mixed with an equal quantity of ſulphate of copper dried, were treated in a convenient apparatus. The acetic acid produced had an odour more penetrating and agreeable: but all the other phenomena were the ſame as with the acetate of lead; that is to ſay, there was an equal developement of carbonic and ſulphurous acids, and charcoal was found in the reſidue.

Which determined the reporters to conclude,

The reporters conclude that this acetic acid is always mixed with ſulphurous acid; of which it cannot be freed entirely by M. Debuc's proceſs; and never has ſo pleaſing an odour as the common kind.

1. That acetic acid formed by the diſtillation of a mixture of ſulphate of copper and acetate of lead, is always mixed with ſulphurous acid, which does not become perceptible till towards the end of the diſtillation.

2. That it cannot be compleatly deprived of this ſulphurous acid by the rectification propoſed by M. Debuc.

3. That the acid itſelf, totally deprived of the ſulphurous acid, is never of ſo lively and agreeable an odour, as that drawn from the cryſtals of the acetate of copper.

4. That it is preferable to dry the ſulphate of copper before it is uſed.

5. That MM. Boddolier and Darac, were miſtaken in ſuppoſing, that no carbonic acid was obtained in this operation.

The difference of acetous and acetic acids probably does not depend on the ſtate of their acidification.

6. Finally that the production of carbonic acid does not any more prove the decarboniſation of the acetous acid in becoming the acetic, than the ſulphurous acid proves the ſuperoxigenation of the vinegar; but on the contrary that it is allowable to conclude, that the difference of theſe two ſubſtances is not cauſed by their ſtate of acidification.

X.

*Account of the Imperial Botanic Garden of Schœnbrunn, in the Vicinity of Vienna.**

The garden eſtabliſhed in 1753, by Francis I.

Put under the care of Adrien Steckhoven, —Vander Schot the firſt gardener —brings to it many exotics from Holland.

IN 1753 the emperor, Francis the firſt, cauſed a portion of ground behind the garden of the caſtle of Schœnbrunn to be prepared for the cultivation of exotics, and of plants remarkable for their rarity or beauty. By the advice of the celebrated Van Swieten, the famous floriſt Adrien Steckhoven was invited to Schœnbrunn from Leyden, who cauſed many green-houſes to be conſtructed there, with a very large and beautiful hot-houſe, and various other buildings. At the ſame time Richard Vander Schot, of Delft, was named firſt gardener, and employed to convey to Vienna a great number of rare and exotic plants, brought up in different parts of Holland, and thus at the end of one year the garden was already rich in valuable plants.

M. Jacquin ſent to America to collect plants.

Account of his proceedings in the Weſt India iſlands.

M. Jacquin, who was then at Vienna, went to viſit the garden of Schœnbrunn, to claſs thoſe plants which had not yet received a ſpecific denomination; on which occaſion he became known to the emperor, who propoſed to him to travel at his expence on the continent of South America, and in the American iſlands, to enrich the garden with plants from the moſt diſtant countries. Accompanied by the gardener Van der Schot, he departed from Vienna in 1754; and in paſſing through Italy was joined by Jean Buonamici and Ferdinand Barculli, who were entruſted with the zoological part of the expedition, by which it was propoſed to improve the royal menagerie, and the cabinet of natural hiſtory at the ſame time. After having viſited the iſlands of Martinico, of Grenada, St. Vincent, St. Euſtatia, St. Chriſtopher, St. Martin, St. Bartholomew, Aruba, Cuba, Caracca, and Jamaica, he returned to Vienna in 1759. From Auguſt 1757 to the middle of 1759, M. Jacquin could do little for the advancement of Science, having been ill of a lientery for four months, of which he was at laſt cured at Jamaica. The war which then commenced between England and France, alſo deranged his travels! The veſſel in which he made his voyage was taken, and he was thus obliged to paſs a conſiderable time, againſt his will, at Montſerrat and the deſert iſland of Gonave.

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* Magaſin Encyclopedique, T.6, p. 552.

The firſt cargo of plants ſhipped, —the ſecond cargo brought by Vander Schot.

Method of preparing the trees for carriage, —and of packing them.

Weight of a tree when packed 100lb.

In the month of Auguſt 1757 the firſt cargo of plants for the garden of Schœbrunn was ſhipped from Martinico, which arrived at Marſeilles. In the month of February, 1757, Vander Schot returned alſo from Martinico, and brought with him from the ſame iſland a great quantity of trees and ſhrubs. All this cargo arrived ſafe, except ſome ſpecimens of *Aleliconia*, which were attacked on the voyage by mice. The trees were of the height of a man, and of the thickneſs of an arm, and ſometimes more. The moſt of them had born fruit in their native ſoil; their tops had been cut off, and only ſome of the principal branches were permitted to remain about two feet in length; the ſhrubs remained in their natural ſtate. To remove thoſe trees from their native earth, a circular trench was dug round each, at a convenient diſtance, in ſuch manner that there might remain attached to their roots as great a maſs of the earth in which they grew as was poſſible. This maſs, which formed a ſort of ball, was entirely wrapped up in leaves of the *Muſa*, ſecured with cords made of the bark of the *hibiſcus tiliaceus*, in ſuch a manner that the earth could not fall out. A ſingle tree packed in this manner, weighed commonly an hundred and odd pounds. The balls of earth were moiſtened a little, with the neceſſary care, and ſuſpended in the air, where the vegetation ſoon became apparent.

Method of tranſporting the packages.

To prevent the earth from being detached from the roots on the way, all the packages were tranſported in barks to the port of St. Pierre, in Martinico; from hence they were ſhipped to Marſeilles, and from thence brought by ſea alſo to Leghorn, and from this port were carried by mules to Schœnbrunn. This was without exception the richeſt cargo of living plants which had ever been brought from the hot countries to Europe.

The third cargo of plants ſhipped, —the fourth, and the fifth.

In the month of Auguſt, 1756, Buonamici ſet off with the third cargo from St. Euſtatia to Leghorn. The fourth cargo departed towards the end of the ſame year. The fifth was ſhipped from Curacao for Amſterdam, and was accompanied by J. A. Veſuntin, who died in Germany of the dyſentery. This cargo was extremely rich in corals and other productions of the ſea, which ſtill form ſome of the moſt precious ornaments of the Imperial Cabinet. In the ſame year, M. Jacquin ſent off the ſixth cargo, from the ſame iſland to Amſterdam. And finally, in January 1759, MM. Jacquin and Barculli departed with the ſeventh cargo from the Havannah, for Ferrol in Spain, and arrived at Vienna in the month of July. This laſt cargo was particularly rich in animals of every ſpecies.

The ſixth cargo ſhipped,—the ſeventh brought by MM. Jacquin and Barculli, 1759.

Thus in the ſpace of a few years the number of plants in the garden of Schœnbrunn was conſiderably encreaſed; for, beſides thoſe which had arrived from America, means were found to make many important acquiſitions in different other countries. In 1765, after the death of Francis the firſt, Maria Thereſa ordered the garden to be conducted on the ſame footing that it was before. In 1780, a little while before the death of this princeſs, it ſuffered a ſmall but irreparable loſs; the gardener, Van der Schot, then very aged, had been confined to his chamber for many weeks by an attack of the gout. Thoſe to whom the management of the plants was entruſted in that period acquitted themſelves with great negligence; in one of the coldeſt nights of that winter, the perſon who ſhould have taken care of the great hot-houſe forgot to keep up the fire. In the morning he thought to repair this neglect by heating it to an unuſual degree; but the ſudden tranſition from cold to heat killed a great number of fine plants, and among others all the cinnamon trees from Martinico, of which the trunks were as thick as a man's arm, the heads very large, and of the greateſt beauty; and alſo deſtroyed the plants *Creſcentia*, *Achras*, *Annona*, *Portlandia*, and a *Coccoloba Grandifolia*, which was 20 feet high, and whoſe leaves were of the ſize of two feet.

The garden receives plants from other countries.

Maria Thereſa patroniſes the inſtitution.

Many valuable plants deſtroyed by the negligence of the attendants with regard to the hot houſe.

This garden alſo ſuffered another loſs. A conſiderable collection ſent from the iſle of France by M. Gere, arrived at Trieſte entirely ſpoiled, the trees all dead, and the ſeeds improlific.

A cargo of plants from the Iſle of France ſpoiled.

At this time the emperor, Joſeph the ſecond, directed M. Jacquin and M. Born to propoſe to ſome men of abilities to undertake a voyage into remote countries. Profeſſor Marter was appointed the conductor of this expedition, and Doctor Stupiez was aſſociated with him, together with the gardeners Boor and Bredemeyer, and the painter Moll. This company of travellers quitted Vienna in the month of April, 1783, and arrived in September at Philadelphia. They travelled over

Prof. Marter and others ſent to collect in America in 1783.

Several fine plants arrive at Vienna from thence, with M. Bredemeyer.

Pennſylvania, Virginia and Carolina. M. Boor along with M. Schopf, made a journey into Florida, and from thence paſſed to the iſland of Providence. M. Bredemeyer returned from Carolina, and paſſing through England, arrived at Vienna in November, 1784, with ſeveral very beautiful plants. Boor, who during his ſtay at the Bahama iſlands had collected many rare plants, returned to Vienna in the month of September, 1785. But the painter Moll and Doctor Stupiez were ſeparated from their fellow travellers.

Bredemeyer ſent out again, —ſearches the iſlands and continent to the river Oronooko.

By the orders of the emperor, M. Bredemeyer, and the gardener Schucht, went towards the end of the year 1784 to rejoin the director of this expedition, M. Marter, who remained all this time in America; they paſſed over many of the great iſlands and a part of the continent as far as the mouth of the river Oronoco.

Many rare plants brought back by him and M. Marter in 1788.

In 1788 they returned by way of Amſterdam to Vienna, and brought back many rare and new plants. M. Marter alſo arrived the ſame year, by the way of London and Bruſſels with a new collection of plants.

M. Boor and Scholl ſent to the Iſle of France.

The emperor had not forgotten the loſs of the plants from the iſle of France, and commiſſioned M. Boor and the gardener Scholl to go there, and touch on their way at the Cape of Good Hope. In the month of May 1786, they arrived at the Cape with the Dutch veſſels; M. Boor remained there till 1787, and then departed by himſelf for the iſle of France and that of Bourbon. In the month of January, 1788, he returned to the Cape with 280 caſes full of rare plants; and on the 20th of July in the ſame year arrived at Vienna with a great number of magnificent vegetables; but as all the caſes could not be brought in the veſſel, the gardener remained at the Cape with the remainder. There has not ſince been any poſſibility of getting them to Vienna, as well as many other plants; and the gardener Scholl remains at the Cape from that time, from whence he has ſent from time to time ſeeds and roots. Beſides this increaſe to the garden, the number of plants was augmented in different manners. Thus, at the ſale of the garden of Schwenk at the Hague, the emperor cauſed all the rare plants to be bought; and likewiſe M. Jacquin, the ſon, when he was on his travels over a great part of Europe, ſent to Schœnbrunn many exotic plants which he found in other gardens.

M. Boor returns with many fine plants, —leaves ſome behind at the Cape with Scholl.

The plants of Schwenk bought.

M. Jacquin, the ſon, ſends many exotics from his travels.

The emperor Joſeph alſo enlarged the hot-houſes, and cauſed others to be built. In order to bring back Scholl the gardener to Vienna, with the plants which remained in his care at the Cape, the emperor Leopold, in 1791, ordered the gardener Bredemeyer and young Van der Schot, (the ſon of him who had been with M. Jacquin in the Eaſt Indies) to ſail to the Iſle of France, where Cere had collected many plants for the imperial garden, and during their return they were to touch at the Cape to take up all thoſe which remained with Scholl. The captain of the veſſel, in which the two gardeners had taken their paſſage put into Malaga; where they diſcovered in time that he had bad intentions with reſpect to them; which obliged them to return to Vienna without performing their commiſſion. After the death of the emperor Leopold, his ſucceſſor Francis the ſecond had an hot-houſe conſtructed, 235 feet long, for the plants of the Cape. A new garden was alſo eſtabliſhed, of which Doctor Hoſt was appointed inſpector, and in which were carefully cultivated all the plants which grew in the ſtates belonging to the houſe of Auſtria.

Emperor Joſeph enlarges the hot houſes and builds others.

Emperor Leopold orders Bredemeyer to the Cape for Scholl,—he is diſappointed of his paſſage.

Francis II. builds an hot houſe 255 feet long for Cape plants.

A new garden added for plants of the Auſtrian ſtates.

By theſe details may be ſeen with what care this juſtly celebrated garden was augmented from the reign of Francis the firſt, and all aſtoniſhment will ceaſe at the riches it contains, and which have furniſhed materials for different magnificent works on Botany, ſuch as the *Icones plantarum rariorum*, publiſhed by M. Jacquin, and above all, that which appeared a few years ago, under the title of *Plantarum rariorum Horti Cæſarei Schœnbrunnenſis deſcriptiones et Icones*, in two volumes folio, containing 150 coloured engravings.

Valuable Botanic publications from the garden of Schœnbrunn.

XI.

Letter from a Correſpondent on the Means of increaſing the Action of Sound on the Organs of ſuch as are partially deaf.

To Mr. NICHOLSON.

SIR

ALTHOUGH I am ſo deaf as not to be able to hear the beating of a watch, unleſs it be put cloſe to the ear, yet, if I place one end of a ſtick, or of a metal rod between my teeth

Sounds imperfectly heard rendered audible through a ſolid applied to the teeth.

and the other end upon the watch, at the diſtance of ſeveral feet, I can hear it very diſtinctly.

The hearing conſiderably aſſiſted by preſſing the external ear forward;

I know only two methods of alleviating the difficulty of hearing articulated ſounds; one is by ſurrounding the ear, with the hand open, and preſſing it forward, the fingers and thumb being fixed behind it; this expedient does more than might be ſuppoſed. Another method is, that of the application of a trumpet; which however, is of but little uſe, conſtructed as it is at preſent. The diſcovery of any inſtrument to facilitate hearing, by being placed in the mouth (probably after the manner of a tobacco-pipe) would be of great importance to a numerous claſs of our fellow creatures, whoſe faculty of hearing is nearly ſufficient for common converſation. If an inſtrument ſhould be invented, which will do any thing at all in this way, our experience in regard to other inventions, encourages an expectation, that improvements will follow: means of aſſiſting human ſight have long been deviſed; little indeed has been done to aſſiſt defective hearing; it is however an object deſerving of more attention than has been beſtowed upon it. If you ſhould be ſo good as to inſert this in your Journal, I indulge a hope, that ſome of your ingenious correſpondents, compaſſionating the unfortunate ſituation of thoſe whoſe hearing is imperfect, may be led to attempt diſcoveries, the reſult of which may be of extenſive utility. It is deſirable to aſcertain the beſt form and ſize of an ear trumpet, and what metal is to be preferred.

Or by a trumpet, though not conſiderably uſeful.

Probability that an inſtrument might be invented to act on the teeth or bones of the head ſo as to magnify ſounds.

I am, Sir,

Your moſt obedient Servant,

A. B.

Reference to ſome diſquiſitions on ſounds in the quarto ſeries of this Journal.

P. S. On referring to my quarto edition of the Journal, vol. IV. page 383, I find ſomething correſponding to my own obſervation. I ſhall be extremely obliged if your humanity ſhould determine you to inſert the above, as it may be a means of exciting inveſtigation on a ſubject which is certainly of great conſequence.

ANNOTATION—W. N.

Deſultory remarks on the modification of ſound by means of ſolid bodies.

A CONSIDERABLE maſs of ſpeculation concerning ſound, and the means of encreaſing its action on the organs of ſenſe is to be found in my annotations on the experiments of Perrole, at p. 416 of vol. I. of the Quarto Series of this Journal. The excellen excellent papers of Mr. Gough, at page 66 and 160 of vol. X. of the preſent Octavo Series, concerning the augmentation of ſounds, and the ſpeaking trumpet have added conſiderably to our knowledge of this ſubject. The memoir of Haſſenfratz on the ſame inſtrument in our Ninth Volume, p. 283, and another, by the ſame author, on the Propagation of Sound, at Vol. XI. p. 127, alſo deſerve to be conſulted. From the whole conſideration of the facts it ſeems as if the ſonorous vibration of the inſtrument were of much more conſequence than has hitherto been ſuſpected; and it ſeems not improbable, that a large ſurface expoſed to the aerial pulſes of ſound, and having a tail of communication to be applied to the teeth, or inſerted in the ear, might have conſiderable effect. The uſe of the external ear, which has excited ſo much diſcuſſion, may, perhaps, be of this kind. The experiment of Dr. Moyes (Philos. Journal, III. 57) of conveying ſound to great diſtance through a ſtring may be added to the other facts; and tends to ſhew that the ſonorous undulation does not require to be tranſmitted through ſuch bodies as are the moſt denſe, uniform, and elaſtic. Leather, or felt, or paſteboard, or various other ſimilar materials, are more frequently obſerved to tremble in the hand at certain particular ſounds than many other denſe bodies.

XII.

Eaſy and Correct Method of verifying the Portion of a Tranſit Inſtrument. By J. S. Butt, *Eſq. Communicated by the Author.*

To Mr. NICHOLSON.

SIR, *Paragon, December* 8, 1805.

A SHORT note having appeared in Mr. Kelly's new edition of Spherics, deſcribing my method of verifying the poſition of a tranſit inſtrument, and thinking an account more in detail may not be unacceptable to your aſtronomical readers, I am induced to trouble you with it, that you may, if you pleaſe, inſert it in your valuable Journal. Introductory note.

Your's reſpectfully,

JAMES STRODE BUTT.

TO

The uſual method of adjuſting a tranſit inſtrument by a circumpolar ſtar.

TO make the line of collimation move in the plane of the meridian, we are deſired to obſerve the tranſits of circumpolar ſtars, and if the intervals between the times of their tranſits are equal, the tranſit inſtrument moves in the plane of the meridian: for the axis and line of collimation being previouſly adjuſted, it muſt paſs through the zenith; and if it divides the circle deſcribed by any circumpolar ſtar, into two equal parts, it muſt paſs through the pole.

— requires the clock to keep time for at leaſt 24 hours.

But here a difficulty ariſes which is a probable alteration, or a want of uniformity in the rates of the clock or watch for ſo long a period as twenty-four hours, or during that portion of time which the obſerver may require to repeat his obſervations, ſo as to be ſatisfied.

Method independant of this rate and of previous right aſcenſion, &c.

A method independant of the rate of a clock or watch for ſo long a time, and alſo entirely of any other previous obſervations of right aſcenſion, is a deſideratum to practical aſtronomers, and alſo to thoſe who occaſionally amuſe themſelves by obſerving time, and the rates of their chronometers, in their preſent improved ſtate; but who may be unacquainted with aſtronomical equations, of preceſſion, nutation, &c.

Rule, obſerve the tranſits of two different ſtars, one above and the other below the pole; which differ only a ſhort time, for example, only a few minutes: at any time afterwards repeat the obſervation upon the ſtars when their ſituations as to the pole are reverſed. If the difference in time be the ſame the tranſit is duly placed, if not it muſt be altered, &c.

Rule, Obſerve the difference of tranſits of any two circumpolar ſtars, that are ſituated nearly in the ſame azimuth, or vertical circle, the one above and the other below the pole: and whoſe difference of right aſcenſion is nearly 180°; (namely)

Obſerve, The tranſit of α caſſiop. above the pole, and immediately after it the tranſit of ε urſæ majoris below the pole, whoſe difference of tranſit is not more than 15 minutes, and for ſo ſhort a time the clock or watch may be ſafely depended on. Then invert the operation, and obſerve the tranſit of α caſſiop. below, and ε urſæ above the pole. If their difference of tranſits is the ſame in both obſervations, the tranſit inſtrument is accurately in the meridian; if not the error may be corrected by altering the poſition of the inſtrument till their difference of tranſit is the ſame in both obſervations.

Should the error be great it may be corrected nearly by any of the theorems now in uſe; (*vide* Wales on Time-Keepers) or half the difference may be ſubſtituted for the error, and by repeated approximation the tranſit inſtrument may be accurately adjuſted.

The advantage of this method is, that you rely upon the ſtars keeping $\frac{99}{100}$ of the time, which would otherwiſe be kept by the clock or watch; and it is of no conſequence whether the obſervations follow one another on the ſame day or week, provided the inſtrument is adjuſted to the ſame point of the horizon previous obſervation, for there is little or no difference in their preceſſion, &c. during an interval of a month.

Advantages. Moſt of the tim is kept by the ſtars, and not by the clock.

Another advantage is, that the obſervations follow each other ſo ſoon, that you are not likely to be diſappointed by a change of weather, for each pair of obſervations is complete as far as it goes, which is not the caſe in the other method, which requiring an interval of twelve hours between each obſervation, a change of weather is more likely to take place.

The ſhort interval inſures againſt change of weather; &c.

A tranſit inſtrument is the baſis of aſtronomy, and whoever has the fixing of it ſhould conſider himſelf independant of every previous obſervation, and acting entirely upon principle, which is not the caſe where the adjuſtment is by previouſly obſerved right aſcenſion, and which require reducing to the day of obſervation; indeed nicely reduced right aſcenſions are not always in the hands of thoſe who may be wiſh to be in poſſeſſion of a ſimple and accurate method of placing a tranſit inſtrument preciſely in the meridian.

Other uſeful remarks.

This method was deviſed and uſed by me ſince 1794, but I have never read or heard of any one uſing the ſame.

J. S. B.

N. B. Proper ſtars in this Lat. are,
α Caſs. and ε Urſæ Majoris.
β Caſs. and δ Urſæ.
γ Caſs. and ε Urſæ.
Alſo the ſtars of Draco and Auriga,
Cepheus and Urſa,
Perſeus and Draco.

A large comet was diſcovered at the Royal Obſervatory Dec. 8, which paſſed the meridian at $6.^{h}$ $24.^{m}$ 7. mean time.

A large comet.

Obſerved right aſcenſion was - 353° 6′ 41″
Declination ſouth - - - - - - 23° 41′ 8″

*** I have ſince heard that this comet was not again ſeen, but is ſuppoſed to have proceeded ſouthward.—N.

XIII.

A Compariſon of ſome Obſervations on the Diurnal Variation of the Barometer, made in Peyrouſe's Voyage round the World, with thoſe made at Calcutta by Dr. Balfour *.

Barometrical obſervations between the Tropics by Lamanon and by Dr. Balfour.

THE firſt of the obſervations here referred to were made by M. Lamanon, an ingenious naturaliſt who accompanied Peyrouſe, and who has given an account of them, (ſee fourth volume of the *Voyage*, octavo edit.), in a letter to M. de Condorcet, dated St. Catharine, 5th November 1785. Dr. Balfour's Obſervations are in the *Aſiatic Reſearches* for 1794, and a ſhort account of them is alſo inſerted in the fourth volume of the *Tranſactions, R. S. Edin.* Hiſt. p. 23.

M. Lamanon's obſervations were made in conſequence of inſtructions from the Academy of Sciences, directing him to keep an exact account of the heights of the barometer in the vicinity of the equator at different hours of the day, with a view to diſcover, if poſſible, the quantity of the variation of that inſtrument, due to the action of the ſun and moon, that quantity being there probably at its *maximum*, while the variations ariſing from other cauſes are at their *minimum*.

Lamanon uſed a marine Barometer of Nairne.

M. Lamanon was provided with one of Nairne's marine barometers, which, he ſays, was ſo little affected by the motion of the ſhip, that it might be depended on to the $\frac{1}{100}$ of an inch. In this barometer, he tells us, that from about the 11th degree of north latitude, he began to perceive a certain regular motion, ſo that the mercury ſtood higheſt about the middle of the day, from which time it deſcended till the evening, and roſe again during the night. As they approached the equator, this became more diſtinctly perceptible; and on the 28th of September, the ſhip being then 1° 17′ north latitude, a ſeries of obſervations was begun, and continued for every hour till the 1ſt of October, at 6 A. M. The following abſtract ſhews the reſult of the obſervations on the 28th and 29th.

Regular diurnal change in lower Lats. than 11°. N.

Twenty eighth of September.

From 4 to 10 A. M.	Barometer roſe	1*l.*	$\frac{9}{10}$
From 10 A. M. to 4 P. M.	fell	1	$\frac{2}{10}$
From 4 to 10 P. M.	roſe	0	$\frac{9}{10}$

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* From the Hiſtory of the Royal Society of Edinburgh, 1805.

Twenty ninth of September.

From 10 (28th) to 4 A. M.	fell	1*l*. $\frac{3}{10}$
From 4 to 10 A. M.	roſe	1 $\frac{5}{10}$
From 10 A. M. to 4 P. M	fell	1 $\frac{3}{10}$
From 4 to 10 P. M.	roſe	1

The obſervation on the 30th were to the ſame effect; and hence it is concluded that at the equator the flux and reflux of the atmoſphere produces in the barometer a variation of about 1 line $\frac{2}{10}$ Engliſh, correſponding, as M. Lamanon remarks, to a height in the atmoſphere of nearly 100 feet. According to Bernouilli, the action of the ſun and moon ſhould produce a tide of ſeven feet, and according to Mr. de la Place, a tide not nearly ſo great.

The effect is greater than might ariſe from the computed atmoſpheric tides.

It ſhould be obſerved, that when theſe obſervations were made, the moon was in her laſt quarter, and the ſun a few degrees to the ſouth of the equator. The latitude on the 28th was 50′ north, and 11′ north on the 29th; in the night between that and the 30th, the ſhip croſſed the line; and on the 30th at noon, the latitude was 42′ ſouth: the longitude all this while between 17° 31′ and 18° 33′ weſt of Paris, by the time-keeper; ſo that the coaſt of Africa, which was the neareſt land, was diſtant about 8° of a great circle, and the American continent about 19°.

Situation of the ſun and moon, and place of the ſhip.

She was *far out at ſea*.

The agreement between theſe, and Dr. Balfour's obſervations at Calcutta is very remarkable. Dr. Balfour found that during the whole lunation, in which he obſerved the barometer from half-hour to half-hour, the mercury conſtantly fell from 10 at night to 6 in the morning; from 6 to 10 in the morning it roſe; from 10 in the morning to 6 at night it fell again; and laſtly roſe from 6 to 10 at night. The *maximum* height is therefore at 10 at night and 10 in the morning, and the *minimum* at 6 at night and 6 in the morning. The only difference is, that in Mr. Lamanon's obſervations, the *minimum* is ſtated to have happened about 4 inſtead of 6. This, however, will not ſeem a very material difference, when it is remembered, that the inſtant when any quantity attains either its greateſt or its leaſt ſtate is not eaſily aſcertained with preciſion. From the obſervations as detailed by M. Lamanon, the time of the *minimum* ſeems to anſwer fully as well to 5 as to 4; ſo that the difference of the reſults is in

Argreement between theſe Obſervations, and thoſe of Dr. Balfour *at Calcutta*.

every view inconſiderable, and their coincidence on the whole, not a little ſingular. The variations in Dr. Balfour's barometer between the neareſt *maximum* and *maximum* is ſometimes about $\frac{1}{10}$ of an inch, though in general conſiderably leſs.

Whether the cauſe which produces land and ſea winds could produce the regular change.

—moſt probably not.

—neither is it likely that it was cauſed by tides in the air, as it does not follow the moon.

In the abſtract of Dr. Balfour's obſervations referred to above, it is remarked, that it ſeems not improbable that theſe variations of the barometer are connected with the reciprocations of the ſea and land winds during the day and night. But whatever may have been formerly the probability of this ſuppoſition, it is entirely deſtroyed by the obſervations of the French navigators. Theſe obſervations were made too far out at ſea to leave room for ſuppoſing that the land winds had any influence on the phenomena to which they refer. It is at the ſame time doubtful, whether thoſe phenomena can be aſcribed to the atmoſpherical tides produced by the ſun and moon, as the *ebbing* and *flowing* of the mercury in the barometer appears to have no dependence on the poſition of thoſe luminaries relatively to one another, but happens, it would ſeem, conſtantly at the ſame hour, in all aſpects of the moon and all ſeaſons of the year. The ſubject is well deſerving of a fuller inveſtigation. We ſhould probably before now have had farther information reſpecting it, if happily the able navigator above-named, and his brave aſſociates, had been deſtined to reviſit their native ſhores. The cruel fate of an expedition ſo well planned, and ſo well appointed for the purpoſes of ſcience, will never ceaſe to be matter of ſincere regret.

Annotation.—W. N.

Probability that the equi-tropical change is cauſed by aſcending and deſcending currents in the atmoſphere.

I have inſerted the foregoing with a view, in ſome meaſure, to afford a compariſon with Mr. Horſburgh's paper on the ſame ſubject, at page 16. It is not without diffidence that I venture to propoſe a conjecture on this ſubject, which in fact requires more conſideration than I can, at preſent, beſtow on it. Its change ſeems to me to be governed by the aſcent of the air which would take place immediately beneath the ſun, if the earth were ſtationary, and the ſurrounding deſcent of the ſame fluid, of which the circumſtances and modifications are ſo well explained by Prieur in his memoir on the morning and evening dew (at p. 86, vol. IV. of our quarto ſeries.) The conſiderations there detailed may be eaſily extended to ſhew alſo that the effects muſt be greatly altered, and, in moſt inſtances, obliterated by the vicinity of land; which even changes the regular trade winds into land and ſea breezes.

XIV.

Abſtract of a Memoir on the Direction and Velocity of the Motion of the Sun and Solar Syſtem. By Dr. HERSCHEL. *From the Philoſophical Tranſactions,* 1805. (A.)

Proper motions of the fixed ſtars.

THE learned author begins his paper by noticing Dr. Maſkelyne's table of the proper motions of 36 ſtars of the firſt magnitude, and conceives that if this table affords proof of motion in ſtars in our immediate neighbourhood, the changes of poſition in minute double ſtars, many of which are only to be ſeen by means of the beſt teleſcopes, likewiſe prove that motions are equally carried on in the remoteſt regions of ſpace.

Deduction of a proper motion of the ſun and its direction.

In 1783, the Doctor deduced from the proper motions of the ſtars, a motion of the ſun and ſolar ſyſtem towards λ hercules; and the opinion he then conceived has been much ſtrengthened by the conſiderations ſtated in the following pages. Should this doctrine be eſtabliſhed, many phenomena may be accounted for, which without it muſt remain inexplicable.

Conſiderations in favour of ſuch a motion.

Though it was propoſed, by the admiſſion of a ſolar motion, to take away many of the proper motions of ſtars, by inveſting the ſun with a contrary one; our author admits that it will reveal a vaſtly greater number of concealed real motions than would be neceſſary to admit, were the ſun at reſt; and that the neceſſity for admitting its motion ought therefore to be well eſtabliſhed.

Reaſons from analogy of the planets, &c.

The motion of ſatellites round their primary planets, and of theſe round the ſun, ſuggeſts the idea of a revolution of the latter round ſome other unknown centre; nor are we without hypotheſes built upon this conjecture.*

The poſſibility of a ſolar motion has been ſhown by the late

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* See Syſtême du Monde de Lambert, p. 152, 158. Alſo Phil. Tranſ for the year 1783, p. 283.

Dr. Wilſon, of Glaſgow, upon theoretical principles; and its probability, from reaſons of the ſame nature, by M. de Lalande.

Probability that the ſun has progreſſive as well as rotatory motion,

The rotatory motion of the ſun, from which the latter concludes a diſplacing of the ſolar centre, indicates a motion of tranſlation in ſpace; for it is not very probable that the mechanical impreſſion which gave the former, ſhould not alſo occaſion the latter. This however can be admitted only as a plauſible hypotheſis, until we attain a knowledge of the cauſe of the rotatory motion.

—and the variable ſtars alſo.

This argument might be ſtrengthened by cloſely obſerving the ſtars which change their magnitudes periodically; for if theſe changes ariſe from a rotatory motion,* a real motion in ſpace may be expected to attend it; and the multitude of theſe ſtars is ſo great, that their concurrent teſtimony is deſirable.

Three ſorts of motions of ſtars.

But ſetting aſide theoretical arguments, the Doctor notices that as all parallactic motions indicate the obſerver not to be at reſt, it may be neceſſary to explain three ſorts of motions, which will frequently be alluded to in the following diſcuſſion.

Parallactic, real, and apparent.

Suppoſe the ſolar ſyſtem to move towards a certain part of the heavens, the ſtars, to an inhabitant of the earth, will appear to move in an oppoſite direction. Let $s\,p$ (*Pl.* II. *Fig.* 1.) repreſent the parallactic motion of a ſtar; which, if the ſtar have no real motion, will alſo be its apparent motion; but if it ſhould have a real motion, which in the ſame time that it could have gone from s to p, would have carried it from s to r, it will be ſeen to move along the diagonal $s\,a$; and $p\,a$, being parallel and equal to $s\,r$ will repreſent its real motion. The triangle $s\,p\,a$ is ſuppoſed to be formed in the concave of the heavens by three arches of great circles, the obſerver being in the centre, and $s\,p$ repreſents the parallactic, $p\,a$ the real, and $s\,a$ the apparent motion of the ſtar. The ſituation and length of theſe arches in ſeconds of a degree will repreſent the direction and quantity of each motion; and calling the ſolar motion S, the diſtance of the ſtar from the ſun d, and the ſine of the ſtar's diſtance from the point towards which the ſun is moving φ; the parallactic motion will be expreſſed thus: $\frac{\varphi . S}{r . d} = s\,p$.

The largeſt ſtars are moſt fit to ſhew the ſun's motion.

A motion of the ſun will occaſion parallactic motions of the ſtars, and *vice verſa*; but to aſcertain if parallactic motions exiſt, ſuch ſtars ſhould be examined as are moſt viſibly affected ſolar motion; which points out the brighteſt ſtars as moſt proper for the purpoſe; for any ſtar may have great real motion, but to have great parallactic motion it muſt be in the neighbourhood of the ſun.

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* See Phil. Tranſ. for the year 1795, p. 68, and our Journal, XI. 271.

The parallactic motions are directed to a point,

Parallactic may be diſtinguiſhed from real motions by their directions: for, if a ſolar motion exiſt, all parallactic ones will tend to a point in oppoſition to the direction of that motion; but real motions will be indiſcriminately diſperſed.

and will combine with the real (angular).

Under theſe diſtinctions, the proper motions of the ſtars, if the ſun be not at reſt, will be parallactic, or compoſed of real and parallactic; the latter caſe conſtituting the apparent motion of the ſtar.

Deduction of the parallactic centre from obſ. on many ſtars.

Dr. H. next deſcribes the meeting of the arches ariſing from a calculation of the proper motions of the 36 ſtars in Dr. Maſkelyne's catalogue on a celeſtial globe, of which ten were made by ſtars of the firſt magnitude, about the conſtellation Hercules; beyond theſe there was no appearance of any other than a promiſcuous ſituation of interſections.—Of the interſecting points, that towards which the ſun moves is denominated the apex of its motion; and as the ſtars will then have a parallactic motion towards the oppoſite point, it has received the appellation of a parallactic centre.

Tabulated reſults.

Interſecting points.	Right Aſcenſion.			Polar diſtance North.		
	°	′	″	°	′	″
1. Sirius and Arcturus, in the mouth of the Dragon - - -	255	39	50	36	41	31
2. Sirius and Capella, near the following hand of Hercules - -	275	9	32	64	21	48
3. Sirius and Lyra, between the hand and knee of Hercules - -	272	23	58	58	23	24
4. Sirius and Aldebaran, in the following leg of Hercules - -	263	25	38	44	39	47
5. Arcturus and Capella, N. of the preceding wing of the Swan -	290	0	58	32	7	23
6. Arcturus and Aldebaran, in the neck of the Dragon - - -	267	2	19	33	57	20
7. Arcturus and Procyon, in the preceding foot of Hercules -	235	3	13	46	21	34
8. Capella and Procyon, S. of the following hand of Hercules -	272	51	49	73	7	56
9. Lyra and Procyon, preceding the following ſhoulder of Hercules	266	46	49	66	48	11
10. Aldebaran and Procyon, in the breaſt of Hercules - -	260	1	29	60	59	34

Confirmation by other ſtars.

As a further confirmation that the parallactic motion may be perceived in the motion of the brighteſt ſtars, Dr. H. examined the interſections made by the proper motions of ſome large ſtars of the next order, with the arches in which the ſtars of the firſt magnitude move, and found 15 which gave ſimilar reſults with the former 10, in pointing out the ſame part of the heavens as a parallactic centre.

This reſult confirmed by double ſtars.

Changes in the poſition of double ſtars indicate the ſame reſult, and may therefore be more eligibly aſcribed to the effect of parallax, than to admit of ſeparate motions in different ſtars: for, if the alterations of the angle of poſition were owing to a motion of the largeſt ſtar in each ſet, ſuch motions muſt, in contradiction to probability, tend nearly to one particular part of the heavens. This argument derives its validity from the ſame ſource with the former, viz. the parallactic motions of at leaſt 28 more ſtars pointing out the ſame apex of a ſolar motion, by their direction to its oppoſite parallactic centre.

and by the harmony of the proper motions deduced from it.

The incongruous mixture of great velocity and extreme ſlowneſs in the proper motions of the ſtars of the ſame magnitude, is removed by the conſideration of parallax from the ſolar motion; and it will be ſeen that there is a general conſiſtency in their motions. The ſame obſervation is alſo applicable with reſpect to the ſidereal occultation of a ſmall ſtar in the Swan.

Inveſtigation of the direction of the ſun's proper motion.

An apex or parallactic center is deduced from the apparent motions of two ſtars, ſuppoſed to have no real motion.

Dr. H. concludes from the foregoing premiſes, that the expediency of admitting a ſolar motion will not be queſtioned, and proceeds to inveſtigate its direction. He begins by proving, that when the proper motions of two ſtars are given, an apex may be found, towards which if the ſun be ſuppoſed to move with a certain velocity, the two given motions may be reſolved into apparent changes ariſing from ſidereal parallax, the ſtars remaining perfectly at reſt. For we muſt not admit more motions than are ſufficient to account for the obſerved changes in the ſituation of the ſtars; and it would be wrong to have recourſe to the motions of two ſtars, when that of the ſun alone may be ſufficient to account for both; which conſideration would be a ſufficient inducement for fixing at once on the calculated apex as well as on the relative diſtances aſſigned to the two ſtars, could other proper motions be, with equal facility, reſolved into ſimilar parallactic appearances: but, when a third ſtar does not direct towards the ſame apex as the former two, its apparent motion cannot be reſolved by the effect of parallax alone; and this difficulty is further enhanced by the number of apices required to ſolve all proper motions into parallactic ones, increaſing, not as the number of ſtars admitted to have proper motions, but, when their ſituation is favourable, as the ſum of an arithmetical ſeries of numbers, beginning at 0, continued to as many terms as there are ſtars admitted.

The author here propoſes an illuſtration of his ſubject by conſidering the three apices, or interſecting points, No. 1, 2, 5, in the foregoing table.

The diſtance of Arcturus from the apex of the ſolar motion is found to be 47° 7′ 6″, and its parallactic motion, which is as the ſine of that diſtance 2.08718″, which is the apparent motion of Arcturus, as eſtabliſhed by obſervation.

Namely Arcturus and Sirius.

Admitting Sirius to be a very large ſtar, at the diſtance of 1.6809 from us, and computing its elongation from the apex of the ſolar motion at 138° 50′ 14.5″, its parallactic motion will be $\frac{\Phi . S}{r . d} = s p = 1.11528''$, which alſo agrees with the apparent motion already aſcertained by obſervation as the proper motion of Sirius.

The diſtance of Capella from the apex of the ſolar motion is 30° 54′ 46″, and admitting the velocity of the ſun towards the before given point, it will occaſion a parallactic motion of Capella, in a direction 89° 54′ 18″ ſouth-following its parallel, amounting to 2.8125″. Capella is here taken for a ſtar of the firſt magnitude, ſuppoſing its diſtance from us to be equal to that of Arcturus.

Hence the parallactic motion of another, viz. Capella, is deduced;

By conſtructing a triangle, the ſides of which repreſent the three motions of every ſtar, not at reſt; one of the ſides, repreſenting the apparent motion, will be equal to 0.4637″; the other ſide, being the parallactic motion, 2.8125″; and the included angle 18° 19′ 27″, from which will be obtained the third ſide, or the real motion of the ſtar, 2.3757″. By the given ſituation of this triangle with reſpect to the parallel of declination of Capella, the angle of the real motion will be had, which is 86° 34′ 11″ north-following the parallel of this ſtar. A compoſition of the parallactic and real motions in the directions, will produce the annual apparent motion, as eſtabliſhed by obſervation.

and by reſolving this into the apparent motion and another, this laſt will be the real (angular) motion, (ſuppoſing the other ſtars to have none.)

It is here obſerved, that although the proper motion of a

third

But it is not fit that all the real motion ſhould be aſcribed to Capella;

but the apex muſt be taken ſo as to leave the real motions as ſmall as poſſible.

third ſtar is accounted for by retaining the ſame apex of the ſolar motion which explained the apparent motions of the other two, yet a great degree of real motion has been aſſigned to Capella, of which Arcturus and Sirius have been altogether deprived; which ſhews that the apex of ſolar motion muſt be ſo fixed as to be equally favourable to every ſtar proper for directing our choice. Hence a problem ariſes, for diſcovering a point whoſe ſituation among three given apices ſhall be ſuch as if the ſun's motion be directed towards it, there may be taken away the greateſt poſſible quantity of proper motion from the three given ſtars. The intricacy of this problem is, that by a change of the diſtance of the apex from any one of the ſtars, its parallactic motion, which is as the ſine of that diſtance, will be affected; ſo that it is not merely the alteration of the angle of direction which is concerned. From the ſolution of this problem, a much more complex one would ariſe, as three ſtars would certainly not be ſufficient to direct the preſent endeavour to find the beſt ſituation of an apex for the ſolar motion.

Apparent motions of ſix bright ſtars

It was before ſhewn that the brighteſt ſtars are the moſt proper for demonſtrating the effect of parallax, and that in ſearching after the direction of the ſolar motion, the aim ſhould be to reduce the proper motions of the ſtars to their loweſt quantities. The ſix principal ſtars, whoſe interſecting arches have been given, when their proper motions in right aſcenſion and polar diſtance are brought into one direction, will have the following apparent motions:

tabulated.

Names of the Stars.	Direction of the apparent Motions.				Quantities of the apparent Motions, per Year
Sirius,	68°	49′	40.7″	South-preceding.	1.11528″
Arcturus,	55	29	42.0	Ditto.	2.08718
Capella,	71	35	22.4	South-following.	0.46374
Lyra,	56	20	57.3	North-following.	0.32435
Aldebaran,	76	29	37.3	South-following.	0.12341
Procyon,	50	2	24.5	South-preceding.	1.23941
				Sum of the apparent Motions,	5.35337

In

Deduction of a ſolar motion that ſhall leave the real motions of theſe the leaſt poſſible.

In ſeeking a ſolar motion, which requires the leaſt motion in the above ſix ſtars, let the line *p a*, *Fig.* 1, which repreſents the real motion, be brought into the ſituation *m a*, and the real motion required will then be at a minimum. If by the choice of an apex for the ſolar motion the angle at *s*, made by the lines *s p* and *s a*, can be leſſened, the quantity of real motion required to bring the ſtar from the parallactic line *s p m* to the obſerved poſition *a*, will alſo be diminiſhed.

A ſingle line may ſhew the direction of parallactic effect in two ſtars, &c.

It has already been ſhewn that when two ſtars only are given, the line *s p* may be made to coincide with the line *s a* of both ſtars, whereby their real motions are reduced to nothing; and that when three ſtars are concerned ſome real motion muſt be admitted in one of them. Now, ſince all parallactic motions are directed towards the ſame center, a ſingle line may repreſent the direction of the effect of the parallax. Therefore, let *s* P or *s* S, *Fig.* 2, ſtand for the direction of the parallactic motion of the ſtars; and as in the foregoing table we have the angles of the apparent motion of ſix ſtars, with the parallel of each, the direction of the line *s* P or *s* S muſt be computed with the parallels of the ſame ſtars, which may be done as ſoon as an apex for the ſolar motion is fixed upon. The difference between theſe angles and the former will give the ſeveral parallactic angles P *s a* or S *s a*, required for an inveſtigation of the leaſt quantity, *m a*, belonging to every ſtar.

Computation, ſuppoſing the ſun to move towards λ Herculis.

The author exemplifies what he here lays down, by ſuppoſing the ſun to move towards λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted with the ſix ſtars already ſelected, he obtains the angles of the parallactic motion with the parallel, the difference between which and the former apparent angles with the parallel of each ſtar gives the angles of the apparent with the parallactic motion, as repreſented in *Fig.* 2. The lines *s a* repreſent the annual quantity of the apparent motions.

When the ſituation of the laſt mentioned angles is regulated as in the figure alluded to, the ſeveral lines *m a* may be drawn perpendicular to S P, and by computation their quantity will be found to be—

Sirius - -	0.65427′
Arcturus - -	1.28784
Capella - -	0.10887
Lyra - - -	0.11281
Aldebaran - -	0.01104
Procyon - -	0.04998
Sum	2.22491

The reſult of this inveſtigation is, that by admitting a motion of the ſun towards λ Herculis, the annual proper motions of the ſix ſtars alluded to, of which the ſum is 5.3537″, may be reduced to real motions of no more than 2.2249″.

A more favourable apex.

The author here obſerves, that although the preciſe place of the beſt apex is difficult to aſcertain, a more favourable one than that above propoſed may be obtained: for, by inſpection of the figure which repreſents the quantities of real motion required, when λ Herculis is fixed upon, it will appear that by a regular method of approximation, the line S P may be turned into a ſituation, wherein all the angles of the apparent motion of the ſix ſtars will be much reduced; and it is evident that the parallactic line S P ſhould be turned more towards the line *s a*, repreſenting the apparent motion of Sirius. He accordingly tries a point near the following knee of Hercules, whoſe right aſcenſion in 270°. 15′. and north polar diſtance 54°. 45′, ſee *Fig.* 3, the quantities required for conſtructing which are found by the ſame method as already deſcribed in *Fig.* 2. By a calculation of the angles and the leaſt quantities of real motion, according to this apex, it appeared that the annual motion of the ſix ſtars was reduced to 1.4594″, which is 0.7655″ leſs than when the apex was λ Herculis.

Its ſituation.

Suppoſition that Sirius may be moſt affected by parallax, as brighteſt;

In the approximation to this point, it appeared, that when the line of the parallactic motion of Sirius was made to coincide with its apparent motion, a certain minimum might be eaſily obtained of the other parallactic motions. But as Sirius has not the greateſt proper motion, the author conceived that another minimum, obtained from the line wherein Arcturus appears to move might be more accurate; as this ſtar from its great proper motion may be more affected by the parallax ariſing from the motion of the ſun. He therefore choſe a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two ſtars which have the greateſt motions.

or Arcturus, as having the greateſt apparent motion.

" H

Apex on this laſt ſuppoſition.

“If the principle of determining the direction of the ſolar motion by the ſtars which have the greateſt proper motion, be admitted”, obſerves the author, “the following apex muſt be extremely near the truth: for, an alteration of a few minutes in right aſcenſion or polar diſtance either way, will immediately increaſe the required real motions of our ſtars. Its place is, right aſcenſion 245° 52′ 30″, and north polar diſtance 40° 22′”. The calculation is delineated in *Fig.* 4. The ſum of the leaſt quantities of real motion in this experiment is 0.95595″, leſs than the former by 0.50843″.

In theſe calculations the author has proceeded upon the principle of obtaining the leaſt poſſible quantity of real motion to aſcertain the moſt favourable ſituation for a ſolar apex, and has proved that the ſum of the obſerved proper motions of the ſix principal ſtars may be the reſult of a compoſition of two other motions; and that if the real motions were reduced to their ſmalleſt poſſible quantities, they would not exceed 0.9559.

If the neareſt ſtars be moſt affected by parallax, their proper motions may alſo be more evident.

The Doctor, however, ſeems to think that theſe real motions may not be brought down to the low quantities mentioned; and proceeds to ſhew that this circumſtance will not affect the arguments he has uſed for eſtabliſhing the method he has adopted; for, although the great proper motions of Arcturus, Procyon, and Sirius, are ſtrong indications of their being affected by parallax, it is not probable that the apparent changes of their ſituations ſhould be entirely owing to ſolar motion; but that their own real motions would have a great ſhare in them; and it is evident that in parallactic motions the diſtance of a ſtar from the ſun is of material conſequence; and as this cannot be aſſumed at pleaſure, we are not at liberty to make the parallactic motion *sp*, *Fig.* 1, equal to the line *sm*; hence it follows that the real motion of the ſtar cannot be from *m* to *a*, but will be from *p* to *a*. If, however, *ma* be a minimum, *pa* when *sp* is given, will alſo be a minimum, and if all the *ma*'s in *Fig.* 4 be minima, the *sp*'s will give the *pa*'s as ſmall as poſſible; which is the point deſired to be eſtabliſhed.

Concluſion. Preferable ſituation of the apex.

In concluding Dr. H. obſerves, that as it is known that proper motions do exiſt, and as no ſolar motion can reſolve them entirely into parallactic ones, we ought to prefer that direction of the motion of the ſun which will take away moſt real motion, and this, as has been ſhewn, will be done when the right aſcenſion of the Apex is 245° 52′ 30″, and its north polar diſtance 40° 22′.

XV.

New Experiments on the Solution of Sulphur in Alcohol, and the various Kinds of Ether. By M. FAVRE *.

Probability that ether would diſſolve more ſulphur than alcohol.

IN my firſt note on the ſolution of ſulphur in alcohol, I announced an intention of examining the ſolvent power of the ſeveral ethers upon this combuſtible; which I had at that time been prevented from, by being obliged to leave Paris for Bruſſells, to take the office of apothecary to the military hoſpital. In the paper alluded to, I hinted that it was probable ethers would diſſolve ſulphur in greater quantity than alcohol; I had been led into this opinion by the reſults obtained from mixing this mineral with alcohol, at various degrees. I obſerved, as already ſtated, that the more alcohol was rectified, the more readily it diſſolved ſulphur; and *vice verſa*, which difference I imagined to proceed from the greater quantum of hydrogen contained in highly rectified alcohol. Knowing ethers to contain leſs carbon and more hydrogen than alcohol, I had no doubt that they would diſſolve a greater quantity of ſulphur. The reſult of the ſeveral experiments, which I made under this impreſſion, I am now about to detail: from which it will be perceived that I was not miſtaken in my conjecture. I ſhall alſo ſubjoin the new experiments, which I made with alcohol, to aſcertain the preciſe quanity of ſulphur it is capable of diſſolving, in order to compare the reſults with thoſe obtained from ethers.

Preparation, &c. of the ethers.

The ethers I employed were prepared with much exactneſs, and according to the methods recommended by profeſſor Fourcroy. I took care, in each experiment, to aſcertain the ſpecific gravity of the ether made uſe of, the quantity of ſulphur diſſolved by it, the various reſults obtained with or without the contact of the ſun's rays, and the properties of ſulphurated ether.

Firſt and ſecond Experiments.

Sulphuric ether by long digeſtion without heat took up nearly

In each of two ſix-ounce matraſſes I put two drachms of the flowers of ſulphur, prepared in the ſame manner as for the experiments mentioned in my firſt note, viz. nicely waſhed, and one ounce of rectified ſulphuric ether, whoſe weight was 0.7396. Having ſecured the mouths of the mattraſſes with luting, I put one in a very light place, and the other in a dark place. I ſhook them every day, and at the end of a month, filtered their contents. On examination the two ſulphurated ethers obtained by theſe operations, preſented the following characteriſtics:

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* Van Mons's Journ. de Chimie, Vol. VI.

One-thirteenth part of ſulphur in the light; and only one-ſeventeenth in the dark.

The colour of the ether expoſed to the light was ſcarcely changed; it had a powerful hydro-ſulphurous ſmell, and its taſte was diſagreeable, and likewiſe hydro-ſulphurous. Mixed with diſtilled water, it precipitated nothing; but I remarked that the water diſſolved a much leſs quantity of it than when pure. In proportion as the ether became volatiliſed, the ſulphur formed a whitiſh ſcum on the ſurface of the liquid, which at length was precipitated to the bottom of the glaſs in which the experiment was made. (I ſhall hereafter mention the quantity of this precipitate.) Put in contact with white metals, it deeply blackened them. (Care muſt be taken in this latter experiment to cloſe exactly the mouth of the veſſel in which the metals are placed in contact with ſulphuric ether, on account of the great tendency of ether to be converted into gas by its attraction of caloric from ſurrounding bodies.) When mixed with a ſolution of acetite of lead, it gave a pretty conſiderable black precipitate.

The ſulphurated ether prepared without light, poſſeſſed all the properties of the other, but in a leſs degree. It alſo was leſs impregnated with ſulphur: for, on a repetition of the experiment, and carefully weighing the products, I found that each ounce of the ether prepared in the light contained 38 grains of ſulphur; whilſt that prepared in the dark held only 29.

Third and fourth Experiments.

Having proceeded as above deſcribed, with nitric ether weighing 0.9088, I obtained an ether whoſe colour was in no degree changed; its ſmell and taſte, though hydro-ſulphurous, were not ſo powerful as thoſe of ſulphurated ſulphuric ether; mixed with diſtilled water, it preſented the ſame phenomena, but depoſited a leſs quantity of ſulphur. It diſcoloured white metals leſs forcibly than the preceding ether;

Nitric ether by the ſame treatment took up nearly one twenty-ſecond part of ſulphur in the light; and only twenty-fourth in the dark.

ether; and, in a word, it had all the qualities of ſulphurated ſulphuric ether, but in a lower degree. It likewiſe contained a leſs quantity of ſulphur; the reſult of the experiment made in the light being but 22 grains of precipitated ſulphur; and 20 for that conducted in darkneſs.

Fifth and ſixth Experiments.

Muriatic ether took up one thirty-ſeventh in the light; and only one fifty-third in the dark.

With muriatic ether, weiging 0.7196, proceeding as already deſcribed, and at the ſame proportions, I obtained a ſulphurated muriatic ether, poſſeſſing all the peculiarities above mentioned, but weaker. It contained only 13 grains of ſulphur, when conducted in the light, and 9⅛ grains when managed in the dark.

Seventh and eighth Experiments.

Acetic ether took up very little ſulphur.

Acetic ether weighing 0.8664, diſſolved but a very ſmall portion of ſulphur, and its qualities were but ſlightly marked. It contained but three grains of ſulphur in an ounce of ether, in the experiment made in the light, and about 1½ grains in that made in the dark.

Ninth Experiment.

Solution of ſulphur in alcohol was leſs charged than that of ſulphuric ether.

Having made the foregoing experiments, I wiſhed to aſcertain the difference exiſting between the ſeveral ethers and alcohol, in reſpect to their capacity for diſſolving ſulphur: I therefore retraced the experiments I had formerly made with alcohol. To avoid the repetition of what has been already communicated in my firſt eſſay, I ſhall here merely ſtate the quantity of ſulphur I have been able to diſſolve, either by ſubmitting the mixture to a heat leſs than ſufficient to cauſe the alcohol to boil, or by expoſing it to the light, or by placing it in a dark place. For theſe experiments I uſed alcohol of 43 degrees.

After digeſting for 12 hours over a gentle fire an ounce of alcohol with two drachms of the flowers of ſulphur, I obtained 23 grains of precipitate.

Tenth and eleventh Experiments.

On leaving ſimilar mixtures, one expoſed to the rays of the ſun, and the other in a place impervious to the light, during a month, and proceeding as already deſcribed, I obtained 16 grains from the firſt mixture, and 13 from the ſecond.

Recapitulation.

After what has here been laid down, it is evident that ſulphuric ether diſſolved the greateſt quantity of ſulphur; for, after frequently repeating the experiment, I found the average to be 25 grains in an ounce. Nitric ether and alcohol at 43 degrees, diſſolved nearly in the ſame proportions; and acetic ether the leaſt of any.

Sulphurated ether is a good medicine.

It has been long a deſideratum in medicine to diſcover a method of adminiſtering ſulphur in a ſtate of extreme diviſion, eſpecially in complaints of the lungs and diſeaſes of the ſkin. With this intent, phyſicians have recommended it to be diſſolved in eſſential oils, and to form what is known in pharmacy under the title of *balſams of ſulphur, terebinthinated, aniſated, &c.* Theſe medicaments have the diſadvantage of giving to the mixtures into which they enter an almoſt inſupportable taſte and ſmell of ſulphurated hydrogen. Sulphurated ether is free from this inconvenience; it may be eaſily mixed with other potions, to which it gives very little ſmell; and as the ſeparation of the ſulphur is only occaſioned by the evaporation of the ether, it may be eaſily prevented by keeping the mixture to which it is added cloſely corked. I have already adopted its uſe with ſucceſs, adminiſtered either upon ſugar, or with any appropriate vehicle: ſeveral phyſicians of my acquaintance, for whom I have prepared it, have likewiſe employed it with advantage: and I hope, ere long, to be able to flatter myſelf as having added an efficacious medicament to the art of healing.

It may be uſed as a teſt for lead in wine.

The ſulphurated ether may be alſo ſucceſsfully employed to detect the adulteration of wine with preparations of lead: in addition to the facility with which this ether precipitates the lead, in the form of a black ſulphur, it poſſeſſes the advantage of introducing nothing into the wine that can deceive as to its quality, which ſometimes happens even to thoſe who are accuſtomed to uſe the ſolution of ſulphur of potaſh.

I am now occupied in the cryſtallization of ſulphur diſſolved in ether, the reſult of which I ſhall loſe no time in laying before the public.

XVI.

On the Utility of ſcientific periodical Publications. In a Letter from Mr. RICHARD WINTER. *To which are added, ſome Experiments of Heat produced by a Blaſt of Air from Bellows.*

To Mr. NICHOLSON.

DEAR SIR,

Periodical works are of modern invention.

THE advantages derived from ſcientific periodic publications, are an acquiſition which former philoſophers were not poſſeſſed of; and it was not until the laſt century they were firſt inſtituted. The rapid progreſs of ſcience and information ſince that period, would be a ſufficient argument in favour of their decided utility, without any reference to ſyſtematic treatiſes publiſhed, of undoubted merit, and ſanctioned by univerſal approbation.

Advantages derived from ſcientific Journals.

To the active and ingenious mind in early life this mode of information is invaluable. Beſides furniſhing new ideas to the young ſtudent; they point out the preciſe ſtate of the different branches of human knowledge; they teach him the neceſſary caution for conducting experiments with vigour and accuracy, inſtead of drawing concluſions from a few inſulated analyſes, or imagining that his data are ſufficiently perfect for eſtabliſhing new ſyſtems. By reading theſe publications it is that he will enlarge his general conceptions, and will learn to emulate the various illuſtrious characters of all the enlightened countries of the world. In theſe treatiſes his views will not be confined to one object, but he will contemplate a ſcene continually varying. The phyſiology and phenomena of the animal and vegetable kingdoms; the actions and re-actions of the different elementary ſubſtances in nature, and their combinations with each other, will paſs in ſucceſſion under his obſervation.

The great phyſical laws which conſtitute and maintain the equilibrium of the world, are inſerted in reſpectable works of this nature as they are diſcovered and demonſtrated, while the errors of former philoſophers are detected and expoſed; by which means he has an opportunity of aſcertaining the value and correctneſs of thoſe works he may be already in poſſeſſion of.

To thoſe who conſult an Encyclopædia for ſcientific matter, theſe publications are of indiſpenſable utility, by continually pointing out the numerous improvements as they become public, and by that means the general ſyſtem of philoſophical knowledge is kept to the level of the exiſting ſtate of diſcovery.

To the mechanic a repoſitory of this kind muſt be highly uſeful, as the receptacle in which he may record his labours and improvements, and ſecure to himſelf the well-earned fame of his diſcoveries, at the ſame time that he derives advantage from others following his example in their contributions to the general fund of ſcience.

In ſhort, there is no claſs of individuals but may profit from this method of extending uſeful knowledge. The ſmall ſum of ſeven-pence or eight-pence a week to any economical perſon is trifling, and there is no doubt but every enquirer will find ſomething of which he may abridge himſelf, in order to become poſſeſſed of ſuch an aſſemblage of facts and opinions. He is as it were making himſelf intimate with a claſs of men whoſe names will be read with admiration by a grateful poſterity. It is only by familiarizing the mind with the ſublime objects of ſcience, and diffuſing them over the face of the earth, that we can expect to eſtabliſh that ſpirit of philanthropy and ſocial order, which is ſo neceſſary to the happineſs of the human race.

I will leave it to your judgment to abridge, or cancel the whole of this paper, as it would perhaps exclude more valuable ſubjects.

I am, Sir,
With the greateſt reſpect,
Your very humble ſervant,
RICHARD WINTER.

21, Bolſover Street,
Dec. 14, 1805.

The following experiments were made in order to aſcertain whether a current of air projected upon a thermometer would increaſe or diminiſh the temperature. I made uſe of a pair of common bellows, the contents of which, when opened, were 95 cubic inches; the diameter of the end of the pipe was

The thermometer raiſed by a blaſt from bellows.

was $\frac{5}{17}$ths of an inch. The thermometer was adapted to Fahrenheit's ſcale, and the reſults of three experiments are exhibited in the following table:

		Number of Blaſts.	Time of blowing.	Therm. roſe.
Exp. 1.	-	425	6 minutes.	4°
2.	-	222	3 ——	3.75
3.	-	217	3 ——	3.7

The current of air was directed againſt the bulb of the thermometer. The diſtance of the pipe out of which the air iſſued, was half an inch from the bulb. The experiments were repeated with every caution poſſible for twelve times, and always with the ſame reſults.

Mr. Dalton obſerved (Philoſ. Journal, III. 160), that the thermometer fell on exhauſting the veſſel in which it was placed, and roſe again on re-admitting the air. It is probable that the riſing of the thermometer in my experiments may be referred to the ſame cauſe, *viz.* the greater capacity of a vacuum for caloric than atmoſpheric air.

XVII.

An Account of two interſecting Rainbows, ſeen at Dunglaſs in Eaſt Lothian in July laſt, was communicated by Profeſſor PLAYFAIR *.

Large rainbow where the ſun was 2° high.

"AT Dunglaſs, where I happened to be in the beginning of July laſt, 1799, our attention was called one evening, a little before ſunſet, to a very large and beautiful rainbow, formed on a cloud which hung over the ſea, and from which a ſhower was falling at a conſiderable diſtance to the S. E. The ſun was about 2° high, ſo that the arch was not much leſs than a ſemicircle, with its higheſt point elevated about 40°. At the point where the northern extremity of this arch touched the horizon, another arch ſeemed alſo to ſpring from the ſea, diverging from the former at an angle of 3° or 4°, on the ſide toward the ſun.

Another interſecting bow, over the ſea.

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* Edin. Tranſ. 1805. Hiſtory, p. 8.

This arch did not exceed 7° or 8° in length; it was of the ſame breadth with the principal bow; it had the colours in the ſame order, and nearly of the ſame brightneſs; or if any difference was diſcernible, it was, that the tranſition from one colour to another was not made with ſo much delicacy in the laſt-mentioned rainbow as in the former.

It was a ſhort portion,

We recollected that a phenomenon ſimilar to this is deſcribed in the *Philoſophical Tranſactions*, as having been ſeen at Spithead, and that it is aſcribed by the gentleman who obſerved it to the reflection of the ſun's rays from the ſurface of the ſea, ſo as to fall on the cloud where the rainbow was formed. This hypotheſis ſeemed to agree exactly with the phenomenon now before us.

and was apparently formed by reflection of the ſun's rays

The accidental rainbow, for ſo it may be called, was ſeen only at the extremity where the principal arch roſe from the ſea, and where of conſequence, the ſun's rays, reflected from the ſurface of the water, at that moment very ſmooth, might fall on the drops of rain. The other parts of the cloud could not receive rays ſo reflected, as the land intervened, and there, accordingly, no veſtige of the accidental rainbow was obſerved.

from the ſmooth water.

The accidental rainbow lay, as was already ſaid, on the ſide toward the ſun, and this is agreeable to the hypotheſis; for the rays that after reflection from the ſurface of the water fell on the drops of rain, muſt have come as from a point as much depreſſed below the horizon, as the ſun was at that inſtant elevated above it. The axis of the accidental rainbow muſt therefore have made with the axis of the principal, an angle equal to twice the ſun's elevation, and its center muſt have been elevated by that ſame quantity above the centre of the other, ſo that if it had been complete, it would have been wholly between the principal rainbow and the ſun.

Its center was above the horizon;

The only cirumſtance in which the appearances did not perfectly correſpond with this hypotheſis, was, that the two rainbows did not interſect one another in the horizon, but rather a little above it. This however, ought to have no great weight, as the reflected image of the ſun cannot have preſented to the cloud a diſk ſo regular and well defined as the ſun itſelf and

but the interſection was not quite as low as the horizon.

the accidental rainbow muſt have ſomewhat participated of this indiſtinctneſs *.

The inclination of the two arcs computed,

When phenomena of this kind occur, it would afford a ſure means of trying the juſtneſs of the explanation, if the inclination of the two bows were obſerved, and alſo the ſun's altitude at the ſame time. Theſe two things are neceſſarily connected; for if we call I the angle of their interſection, E the elevation of the ſun, and S the angle ſubtended at the eye by the ſemidiameter of the rainbow, if complete, an angle which is conſtantly the ſame, and nearly equal to 42°, it is eaſy to infer from ſpherical trigonometry, that $\text{ſin}\ \frac{1}{2} I = \frac{\text{ſin E}}{\text{ſin S}}$.

and was a little more than the eſtimate.

Computing from this formula, the inclination of the two bows in the preſent inſtance comes out nearly 5°; ſomewhat greater than I was inclined to eſtimate it by the eye.

Phenomena of this kind can but rarely occur, as the neceſſary conditions will not often come together. The principal rainbow muſt be over the ſea; the ſea itſelf muſt extend ſomewhat on the ſide toward the ſun; it muſt be ſmooth and tranquil, and the ſun ſo low that the light reflected from the water may be conſiderable. Were it ever to happen that the accidental bow was completely formed; the effect could not fail to be very ſtriking.

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[*] As the place of interſection will lie in a plane paſſing through the eye of the obſerver and parallel to the plane of reflection; does not this fact afford ground for a ſuſpicion that the reflection, at this low altitude, was made, not from the ſurface of the ſea, but from that of the ſtratum of vapour which occaſions looming, and has been ſo well treated of by Dr. Wollaſton and others, (ſee our Journal, VI. 46, and elſewhere), and that this ſtratum was higher farther out at ſea than near the coaſt?—N.

XVIII.

Notice of a Collection of Memoirs which have lately appeared at Paris, being Part of a Work on which the celebrated Lavoiſier was employed till the lamented Cloſe of his Life; with a Tranſlation of that Memoir, in which he claims the modern Theory of Chemiſtry as his own excluſive Diſcovery. Received from Mr. W. A. Cadell, at Paris.

To Mr. NICHOLSON,

SIR, Paris, Oct. 27, 1805.

I HAVE tranſlated the two following paſſages (pages 4 and 5) from a work which has lately appeared in two volumes octavo, entitled *Memoires de Chimie*. They will prove intereſting to the readers of your Journal. The firſt is the notice prefixed to the work by Madame Lavoiſier, (now counteſs of Rumford) it is written with the eloquence of real feeling, and I refer to it for an account of the nature of the work; the ſecond proves completely that the new theory of chemiſtry is due to M. Lavoſier alone. I alſo ſend you the titles of the papers of which the work is compoſed. I am,

See introductory letter.

Your very humble ſervant,

W. A. CADELL.

CONTENTS OF THE TWO VOLUMES.

PART. I.—*General Views on Caloric: its Effects; the Manner of meaſuring it, and the Formation of Liquids and Fluids.*

Contents of the memoirs arranged by Lavoiſier.

1ſt mem. on caloric, by *Lavoiſier*. Mem. Ac. des Sci. 1777.

2. On caloric, and the means of meaſuring its effects. *ib.* 1780. *Lavois. et Laplace.*

3. Supplement to the preceding. *Lavois. and Laplace.*

4. On ſome of the principal phenomena of chemiſtry.—*Seguin.* Soc. Philom. 1790.

5. On the natural zero. *Seguin.* Annal. de Chim. 1790.

6. On the effects of heat in dilating metals and glaſs, &c. *Laplace and Lavoiſier.*

7. On the paſſage of ſolids to a ſtate of liquidity by means of heat. *Lavoiſier.*

8. On the action of heat on liquids from their freezing point to that of their vaporization. *Lavoiſier.*

Contents of the memoirs arranged by Lavoiſier.

9. On the combination of heat with different evaporable ſubſtances and the formation of ſeveral fluids. *Lavoiſier.* Mem. Ac. Sci. 1777.

10. On the electricity that is abſorbed by bodies that paſs to the ſtate of vapour. *Lavoiſier and Laplace*, M. A. S. 1781.

11. On the action of heat on ſome aerial fluids from the freezing to the boiling point. *Guyton and Duvernois.*

12. On ſome ſubſtances which are conſtantly in the ſtate of aerial fluid at the ordinary temperature and preſſure of the atmoſphere. *Lavoiſier.*

13. Memoir on ſome liquids which can be obtained in an aerial form at a degree of heat a little above the mean temperature of the earth. M. A. S. 1777.

14. General views concerning the formation and conſtitution of the atmoſphere.

15. On the cauſe of ſome of the principal phenomena of meteorology.

PART II. *On the Decompoſition of atmoſpheric Air, its Analyſis and the Converſion of its Principles into the ſolid or liquid State.*

SECTION I. *On the Decompoſition of Air by metalic Subſtances and the Formation of Oxids.*

1. Memoir on the action of mercury upon atmoſpheric air. *Lavoiſier.* In pars. in M. A. S. 1777, p. 186.

2. On the decompoſition of atmoſpheric air by the oxidation of lead and tin performed by means of a burning glaſs under a glaſs receiver. *Lavoiſier.* Opuſc. Chim. chap. 6. pub. in 1773.

3. On the oxidation of tin in cloſe veſſels, &c. *Lavoiſier.* Read in 1774.

4. On the decompoſition of atmoſpheric air by iron. *Lavois.*

5. Hiſtorical details on the cauſe of the augmentation of weight that metals acquire when heated with contact of air. *Lavoiſier.* It is the paper of which I ſend you the tranſlation.

SECITON II. *on the Decompoſition of Air by ſimple inflammable Subſtances which form Acids by their Combuſtion.*

1. Memoir on the decompoſition of air by phoſphorus, and the formation of phoſphoric acid. *Lavoiſier.* Opuſc. Chim.

2. Sup-

Contents of the memoirs arranged by Lavoiſier.

2. Supplement to the preceding paper. *Lavoſier.* M. A. S. 1777.

3. Proving that caloric diſengaged from vital air during combuſtion is not poſſeſſed of weight ſuſceptible of being eſtimated.

Proceſs uſually employed for obtaining phoſphorus, phoſphoric acid, and phoſphoreous acid. *Seguin.*

5. Memoir on the combuſtion of phoſphorus employed as an eudiometer. *Seguin.*

9. On the decompoſition of air by ſulphur, the formation of ſulphureous and ſulphuric acid, and the uſe of ſulphurets in eudiometry. *Lavoiſier.*

7. On the proceſs employed in commerce to obtain the ſulphureous and ſulphuric acids. *Seguin.*

8. On the decompoſition of air by charcoal and the formation of carbonic acid. *Lavoiſier.* M. A. S. 1781.

2. On the formation of nitric acid by the immediate combination of azotic gas and vital air. *Seguin.*

10. On the eudiometer compoſed of nitrous gas. *Seguin.*

SECTION III. *On the Decompoſition of Air by thoſe ſimple inflammable Subſtances which do not form Acids by their Combuſtion.*

1. Mem. Account of the laſt experiments on the decompoſition and recompoſition of water. *Lavoiſier* Journal Polytip. February, 1786.

2. Shewing that water is not a ſimple ſubſtance, but a binary combination of hydrogen and oxigen. *Lavoiſier.* read in 1783.

3. Shewing by the decompoſition of water that it is not a ſimple ſubſtance, and that there are ſeveral means of obtaining in abundance, the hydrogen gas, which is one of its elements. *Lavoſier* M. A. S. 1781.

4. Report on the paper of Seguin, which treats of the combuſtion of hydrogen gas with vital air. *Lavoiſier, Laplace, &c.*

5. On the combuſtion of hydrogen gas in cloſe veſſels. *Fourcroy, Vauquelin,* and *Seguin,* read in 1790.

PART IV. *On the principal Phenomena of the Animal Œconomy.*

1. Mem. Experiments on the reſpiration of animals, and the change which takes place in air in the lungs. M. A. S. 1777, p. 185.

2. The alterations that the air undergoes during reſpiration. *Lavoiſier*, read in 1785.

3. Memoir, report on a paper of Seguin's concerning reſpiration and animal heat. *Maquer and Fourcroy.*

4. On reſpiration and animal heat. *Seguin.*

5. On the reſpiration of animals. *Seguin and Lavoiſier*, read in 1790.

Notice prefixed to the Work (by (Mad. Lavoiſier) counteſs of Rumford.

Intention of Lavoiſier to republiſh his memoirs,

In the year 1792 M. Lavoſier had formed the deſign of making a collection of all his memoirs which had been read at the academy during the twenty years preceding. This would have formed in ſome degree the hiſtory of modern chemiſtry.

In order to render this hiſtory more intereſting, and more complete he had propoſed to inſert the memoirs of thoſe, who having adopted his theory, had publiſhed experiments in ſupport of it.

in eight volumes,

This collection was to have been compriſed in about eight volumes.

All Europe is acquainted with the cauſe which prevented their completion.

Parts recovered.

The portion that have been recovered are, the firſt volume almoſt entire, the whole of the ſecond, and ſome ſheets of the fourth.

Several men of ſcience expreſſed a deſire for their publication: this was received with heſitation—it is difficult not to be under apprehenſions when we are intruſted with the power of publiſhing the unfiniſhed work of a man juſtly celebrated. When we have loſt the object of our affections and veneration, we ſhould employ an impartial criticiſm, in order to offer to the public thoſe of his works only which may augment his fame.

Madame Lavoiſier has printed them.

We ſhould have perſiſted, and theſe fragments would not have appeared, had they not contained a memoir of M. Lavoiſier (inſerted below page 5) in which he reclaims the modern theory of chemiſtry as belonging to himſelf, and ſtates the facts in ſupport of his claim.

It is conſequently a duty towards him to fix the opinion of men of ſcience concerning this point.

Lavoiſier was employed on this

Their indulgence is requeſted for the errors that may exiſt in ſome other parts of the collection. It will be granted when they are informed that the greateſt part of the proof ſheets were reviſed in the laſt days of the author's life; and that whilſt he knew that his aſſaſſins were premeditating his death, M. Lavoſier, calm and intrepid, employed his laſt moments in a work which he conſidered as uſeful to ſcience, and gave a great example of that ſerenity which a virtuous and enlightened man can preſerve in the midſt of the moſt ſevere calamities.

work in the laſt moments of his life.

PART II.—SECT. I. *Fifth Memoir*, (Tom. II. p. 78.)

Hiſtorical Details concerning the Augmentation of Weight which the Metals acquire when heated with Contact of Air. (By Lavoiſier.)

Limit of this hiſtorical memoir.

IT is not my object in this paper to give a compleat hiſtory of the different opinions that have been ſucceſſively adopted by the chemiſts and natural philoſophers on the cauſe of the augmentation of weight in metals expoſed to the action of heat; ſuch a hiſtory would only ſerve to ſhew how much the minds of men are ſuſceptible of being led aſtray when they give themſelves up to the ſpirit of theory, and how eaſily we are deceived by reaſoning, if it is not perpetually rectified by experiment. John Rey, a phyſician (*medecin*) little known is one of the firſt authors who has written on this ſubject; he lived in the beginning of the 17th century at Bugue in Perigord, and kept a correſpondence with the ſmall number of perſons who cultivated the ſciences at that time.

John Rey an early writer on combuſtion.

His philoſophy far exceeded that of his cotemporaries.

Neither Deſcartes nor Paſcal had yet appeared; the vacuum of Boyle, and that of Toricelli, the cauſe of the aſcent of liquids in tubes void of air, were unknown; experimental philoſophy did not exiſt; a profound darkneſs reigned in chemiſtry. Nevertheleſs, J. Rey, in a work publiſhed in 1630, with a view of determining the cauſe of the augmentation of weight which takes place in lead and tin during their oxidation, diſplayed views ſo profound, ſo analogous to the facts which have been ſince confirmed by experiment, ſo conformable to the doctrines of ſaturation and affinity, that for a long time I could not help ſuſpecting that the eſſays of J. Rey had been compoſed at a much later period than that announced on the title page of the book.

He contends that metals gain weight from the air in oxidation.

J. Rey, after having refuted ſucceſsfully, not by facts (for at that time the art of making experiments was in its infancy) but by concluſive reaſoning, the different cauſes to which the augmentation of weight of metallic oxides might be attributed, expreſſes himſelf as follows in his 15th eſſay: "to this queſtion then, ſupported on the grounds already mentioned, I anſwer and maintain with confidence, that the increaſe of weight ariſes from the air of the veſſel, which is condenſed, rendered heavy, and adheſive, by the violent and long continued heat of the furnace; this air mixes itſelf with the calx (frequent agitation conducing) and attaches itſelf to the minuteſt molecules, in the ſame manner as water renders heavy ſand which is agitated with it, and moiſtens and adheres to the ſmalleſt grains.

He oppoſes other current opinions.

J. Rey combats in this work the opinion of Cardan (*lib. 5. de ſubtilitate*) on the augmentation of weight of metallic oxides; that of Scaliger, that of Cœſalpinus, who aſcribed this augmentation to a ſoot condenſed and reflected by the furnace, which ſoot, according to their opinion, fell down upon the metal. He ſhews likewiſe that the augmentation of [illegible] proceeds neither from the veſſel, nor from any emanation of the charcoal, nor from the humidity of the air. It is difficult to conceive how J. Rey could attain to theſe concluſions by the force of reaſoning alone, without experiment, and ignorant as he was of many of the preliminary date.

His doctrines were not received by Boyle,

It appears that towards the end of the laſt century, when Boyle and ſome of his cotemporaries created the new ſcience of natural philoſophy, of which the ancients had not the ſlighteſt notion, the work of J. Rey was entirely forgotten.— Boyle, in his treatiſe on the weight of flame and of fire, publiſhed in 1670, that is 40 years after the publication of Rey's work, makes no mention of it; proceeding upon ſome illuſory experiments, he ſtill maintained at that time that the augmentation of weight which the metals acquire by their oxidation ariſes from the fixation of fire.

—nor by Lemery.

Lemery, who was an exact and ſcrupulous obſerver, embraced the ſame opinion: he attributes the oxidation of metals and their augmentation in weight which accompanies that operation, to the combination of igneous particles with the metal.

Opinion of Charras,

Charras, cotemporary of Lemery, aſcribed that augmentation to the acids of the wood and charcoal, which as he ſuppoſed penetrated the veſſels and entered into combination with the metals. Since that time the ſame acid of wood and [illegible] coal has re-appeared under the name of *acidum pingue*, igneous acid, and under ſeveral other denominations which it would be ſuperfluous to enumerate.

—and of Staahl.

Staahl could not be ignorant of the fact that metals expoſed to heat acquire an increaſe of weight; and yet he not only did not attempt to explain it, but alſo the ſyſtem under which he claſſed the whole of the chemical phenomena, and which after him has been ſo much extended, is abſolutely in contradiction with this capital fact.

Staahl ſuppoſed that the metals are compoſed of a metallic earth, and an inflammable principle, which he named phlogiſton; he pretended that they loſt this principle by their oxidation, and that they could not return to the metallic ſtate unleſs the phlogiſton they had loſt was reſtored to them.

Difficulties of the phlogiſtic ſyſtem.

It was difficult to imagine how the metals acquired weight, whilſt, according to Staahl's doctrine, they loſt a part of their ſubſtance, and on the other hand, how they diminiſhed in weight, when they recovered one of the principles which they had loſt; it was one of the chief difficulties that could be propoſed againſt the theory of Staahl, this difficulty however, has not hindered the theory from having a ſucceſs of limited duration.

Morveau's endeavours to remove them.

Guyton Morveau has made ſome unſucceſsful efforts to palliate this contradiction, in his diſſertation on this ſubject, under the title of *Degreſſions Academiques*; he ſuppoſes that phlogiſton is lighter than atmoſpheric air; and he concludes that all bodies that acquire phlogiſton ſhould loſe a part of their weight; that, on the contrary, thoſe which loſe phlogiſton ſhould augment in weight. This explanation would have been tenable, had the augmentation of weight acquired by the metallic oxides been equal only to the weight of the air diſplaced; or, which is the ſame thing, if it had diſappeared on weighing in vacuo; but the augmentation is much too conſiderable to admit of being attributed to that cauſe, ſince in ſome metals it exceeds one third of their weight. It is neceſſary then, either to give up the explanation of Guyton Morveau, or to ſuppoſe that phlogiſton has a negative gravity, a tendency to recede from the centre of the earth, a ſuppoſition incompatible with all the facts admitted by the diſciples of Staahl.

Hiſtory of the authors experiments.

Such was the ſtate of the ſcience when a ſet of experiments undertaken in 1772, upon the different kinds of air or gas which are diſengaged in efferveſcence,' and a great number of other chemical operations, diſcovered to me demonſtratively the cauſe of the augmentation of weight that the metals acquire when expoſed to heat. At that time I was not acquainted with J. Rey's work upon the ſubject, publiſhed in 1630; and had I known it, I ſhould have conſidered his opinion in the light of a vague conjecture, which did honour to the genius of the author, but required the attention of chemiſts in order to aſcertain the truth of the opinion by experiment. I was young, I had newly entered the liſts of ſcience, I was deſirous of fame, and I thought it neceſſary to take ſome ſteps to ſecure to myſelf the property of my diſcovery. At that time there exiſted an habitual correſpondence between the men of ſcience of France and thoſe of England; there was a kind of rivalry between the two nations, which gave importance to new experiments, and which ſometimes was the cauſe that the writers of the one or the other of the nations diſputed the diſcovery with the real author; conſequently I thought it proper to depoſit on the 1ſt of November 1772, the following note in the hands of the ſecretary of the Academy. This note was opened at the meeting of the 5th of May following, and mention of theſe circumſtances marked at the top of the note. It was in the following terms:

He finds that ſulphur and phoſphorus gain weight by combuſtion, and that the gain is from air abſorbed. He infers that the phenomenon is general and diſengages air from litharge on reducing it in cloſed veſſels.

"About eight days ago I diſcovered that ſulphur in burning, far from loſing, augments in weight; that is to ſay, that from one pound of ſulphur much more than one pound of vitriolic acid is obtained, without reckoning the humidity of the air; phoſphorus preſents the ſame phenomenon; this augmentation of weight ariſes from a great quantity of air, which becomes fixed during the combuſtion, and which combines with the vapours."

"This diſcovery, which I confirmed by experiments which I regard as deciſive, led me to think that what is obſerved in the combuſtion of ſulphur and phoſphorus might likewiſe take place with reſpect to all the bodies which augment in weight by combuſtion and by calcination, and I was perſuaded that the augmentation of weight in the calces of metals proceeded from the ſame cauſe. The experiment fully confirmed my conjectures; I operated the reduction of litharge in cloſe veſſels with Hales's apparatus, and I obſerved that at the moment of the paſſage of the calx into the metallic ſtate, there was a diſengagement of air in conſiderable quantity, and that this air formed a volume at leaſt 1000 times greater than that of the litharge employed. As this diſcovery appears to me one of the moſt intereſting which has been made ſince Staahl, I thought it expedient to ſecure to myſelf the property, by depoſiting the preſent note in the hands of the ſecretary of the academy, to remain ſecret till the period when I ſhall publiſh my experiments."

(Signed) LAVOISIER.

Paris, 1ſt November, 1772.

Whence he vindicates his right to the modern theory of combuſtion in 1772.

Comparing this firſt note with that which I had depoſited at the academy the 20th of October preceding on the combuſtion of phoſphorus, with the paper which I read at the academy of the public meeting of Eaſter, 1773, and laſtly with thoſe that I have publiſhed ſucceſſively, it is eaſy to perceive that I had conceived ſo early as the year 1772 the general idea of the theory of combuſtion which I have ſince publiſhed.

—which was not adopted by other chemiſts till many years afterwards.

This theory which I have conſiderably developed in 1777, and which almoſt at that period I brought to the degree of perfection in which it is at preſent, was not begun to be taught by Fourcroy till the winter 1786-1787; it was not adopted by Guyton Morveau till a later period; and Berthollet wrote ſtill in the language of the phlogiſtic doctrine in 1785. This theory then is not, as I hear it called, the theory of the French chemiſts; it is *mine*, and it is a property which I reclaim before the tribunal of my cotemporaries and of poſterity. Others undoubtedly have contributed to its perfection, but I hope that no one will diſpute with me, all the theory of oxidation and combuſtion; the analyſis and decompoſition of air by metals and inflammable bodies; the theory of acidification; more accurate knowledge on the nature of a great many acids, and particularly the vegetable acids; the firſt notions on the compoſition of vegetable and animal ſubſtances; the theory of reſpiration, in which Seguin co-operated with me; the preſent collection will preſent all the papers on which I found my claims; the reader will judge.

The claim ſpecifically ſtated.

ANNOTATION.—W. N.

Notice of the early inventors

It was my intention to have pointed out how far the earlier chemiſts, as well as ſome of the contemporaries of this deſerv-

edly

of the theory of combuſtion.

edly celebrated philoſopher, are intitled to rights which will greatly modify the unqualified claim he has made. I cannot now ſay, whether Rey did, or did not *make experiments*, but whether he did or not, he certainly muſt have founded his introductions upon facts; and between the obſervation of well eſtabliſhed facts, and the making of direct experiments there ſeems to be no eſſential difference. How it has happened that the great Robert Hooke, who had inveſtigated the modern theory of combuſtion in 1664, and publiſhed it in an ample detail on his micrographia in 1675*, and John Mayow, who ſoon afterwards, or about the ſame time eſtabliſhed the ſame doctrine, and extended it to phyſiological reſults, are overlooked by our author, appears to require ſome diſcuſſion. I ſhall take an early opportunity of reſuming this ſubject.

XIX.

On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Eſq. F. R. S. Profeſſor of Chemiſtry in the Royal Inſtitution †.

Acid of borax very uſeful in analyſis.

I HAVE found the boracic acid a very uſeful ſubſtance for bringing the conſtituent parts of ſtones containing a fixed alkali into ſolution.

It combines with earths by ignition and quits them to mineral acids.

Its attraction for the different ſimple earths is conſiderable at the heat of ignition, but the compounds that it forms with them are eaſily decompoſed by the mineral acids diſſolved in water, and it is on this circumſtance that the method of analyſis is founded.

Proceſſes.

The proceſſes are very ſimple.

Pulverize the ſtone and fuſe with two parts boracic acid.

100 grains of the ſtone to be examined in very fine powder, muſt be fuſed for about half an hour, at a ſtrong red heat, in a crucible of platina or ſilver, with 200 grains of boracic acid.

Digeſt with weak nitric acid.

An ounce and half of nitric acid, diluted with ſeven or eight times its quantity of water, muſt be digeſted upon the fuſed maſs till the whole is decompoſed.

Evaporate.

The fluid muſt be evaporated till its quantity is reduced to an ounce and half or two ounces.

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* Copied in our Journal quarto ſeries III. 479.

† Phil. Tranſ. Part II. for 1805.

If the ſtone contain ſilex, this earth will be ſeparated in the proceſs of ſolution and evaporation; and it muſt be collected upon a filter, and waſhed with diſtilled water till the boracic acid and all the ſaline matter is ſeparated from it.

Silex if preſent will ſeparate.

The fluid, mixed with the water that has paſſed through the filter, muſt be evaporated, till it is reduced to a convenient quantity, ſuch as that of half a pint; when it muſt be ſaturated with carbonate of ammonia, and boiled with an exceſs of this ſalt, till all the materials that it contains, capable of being precipitated, have fallen to the bottom of the veſſel.

Precipitate the reſt with carbonate of ammonia.

The ſolution muſt then be ſeparated by the filter, and the earths and metallic oxides retained.

It muſt be mixed with nitric acid till it taſtes ſtrongly ſour, and evaporated till the boracic acid appears free.

Add nitric acid to the clear liquid.

The fluid muſt be paſſed through the filter, and ſubjected to evaporation till it becomes dry; when, by expoſure to a heat equal to 450° Farenheit, the nitrate of ammonia will be decompoſed, and the nitrate of potaſh or ſoda will remain in the veſſel.

Separate the boracic acid by evaporation.

Decompoſe the nitrate of ammonia by heat.

It will be unneceſſary for me to deſcribe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have uſed the common proceſſes. I have ſeparated the alumine by ſolution of potaſh, the lime by ſulphuric acid, the oxide of iron by ſuccinate of ammonia, the mangan•ſe by hydroſulphuret of potaſh, and the magneſia by pure ſoda.

XX.

Some Facts and Speculations on the luminous Phenomena of Electricity. W. N.

ABOUT eighteen years ago, I was conſiderably occupied in experiments upon electricity, many of which were communicated in 1789, to the Royal Society, and were publiſhed in the tranſactions. In the twenty-third ſection of that communication, ſome account is given of certain changes which take place in the luminous appearance of metallic balls when electrified; but the phenomena were not delineated, becauſe I reſerved them for another opportunity. After ſo long an interval

Communication to the Royal Society on electricity.

terval of time, I now preſent them to the reader from my notes, and the ſketch then made.

Three appearances of an electrified ball. It gives flaſhes; is then luminous, and then gives flaſhes of another kind.

Sept. 19, 1787. A ſmall ball in the ſtate of electricity called poſitive, threw out flaſhes or ramified ſparks; and when the intenſity was encreaſed, the ball itſelf became luminous, at the ſame time emitting the flaſhes. When the electricity was ſtill more ſtrongly excited the flaſhes ceaſed, and a circle of light, extending about 15 degrees round the point fartheſt from the ſtem, was ſeen on the ball, and a ſtrong wind proceeded from it.

Experiment with a ball 1½ inch diameter.

Deſcription.

A ball of one inch and a half diameter was uſed; and electricity communicated by means of a cylinder nine inches diameter, having its cuſhion eight inches long. The excitation was ſtrong enough, by ſlow turning with a ſingle winch, to throw out large bruſhes of light. When the rotation was quicker, the flaſhes diſappeared, and the circle of light was ſeen, having a bright ſpeck moving irregularly round in its periphery. Quicker turning threw out bruſhes of light very different from the others: Theſe were leſs luminous in the branches; many ſtarted out at once with a hoarſe ſound. They were greeniſh at the point or ſurface of the ball, reddiſh in the ſtem, and ramified ſooner. Half a dozen were ſometimes ſeen flaſhing out at once.

Experiment with a much ſmaller ball. It became luminous, and acted like a point.

A ball of four tenths of an inch in diameter was uſed. Moderate excitation produced a denſe bruſh of light about two inches in length. With ſtronger electricity the bruſh diſappeared, and the upper half of the ball became luminous. When the excitation was ſtill ſtronger, more than half of the ball was luminous, as repreſented *Fig. 3, Plate I.* and ſometimes a ramified flaſh ſtruck out from the top. Other flaſhes were ſometimes ſeen ſideways when the electricity was ſtrongeſt of all; but this happened ſeldom.

The light was faint, and ſeemed to be about twice the diameter of the ball. It extended more than half way down, and ſpread moſt ſideways.

A large ball 2½ inch. diam.

When a larger ball of two and a half inch diameter was uſed, the bruſhes of light flew out from three or four ſtems together to the length of about ſix or ſeven inches, making a hoarſe noiſe; but they could not be made to diſappear, though they ſeemed now and then to ceaſe for a moment when the turning was moſt vigorous.

Modification of the phenomena,

The next day, when the excitation was very nearly, but not quite, as ſtrong, it was obſerved that the order of theſe appearances could be effected by the aſſiſtance of a metallic point. Plentiful bruſhes were thrown out from a three inch ball, but they could not be made to diſappear. When a pointed wire or a ſmall metallic ball was preſented, the effects were as follow:

by the vicinity of a point or ſmall ball.

The point being at a great diſtance, the root of the bruſh had a luminous circle of lambent light round it on the ſurface of the ball. When the point was nearer, the bruſh diſappeared, and nothing was ſeen but an exceedingly bright ſpeck on the ſurface of the ball, which was ſometimes ſtationary and ſometimes moved about. When the point was ſtill nearer, the ſpeck threw out ramified ſparks of the ſecond kind, at the ſame time that a lambent luminous circle appeared. The ſpeck was never in the center of the circle, but moved at a diſtance round the circle, irregularly, ſometimes the one way and ſometimes the contrary, and was ſometimes ſtationary.

More particular deſcription of the luminous appearances.

Theſe two orders of bruſhes were entirely the ſame as thoſe of the day before. The luminous bruſh which firſt appeared had a ſtraight ſtem, then a broken or leſs luminous part, beyond which looſe cotton-looking fibres flew off in radial directions, as at *Fig.* 1, *Pl. I.* The latter ramified ſparks had a ſtraight central ſtem, out of which well defined branches iſſued nearly at right angles. They much more cloſely reſembled a tree bare of leaves.

The ſecond bruſh was not larger, but rather leſs in its dimenſions than the firſt.

All the phenomena viſible at once.

When the ball of four-tenths of an inch was held at a certain diſtance from the two and a half inch ball, when electrified, the firſt kind of bruſh was ſeen on the ſide fartheſt from the ſmall ball, at the ſame time that the ſecond kind of ſpark or bruſh flew out towards the ſmall ball, and the lambent luminous appearance was ſeen on the ſurface.

Theſe are the general facts; but I have no doubt but they would preſent many modifications upon being repeated.

Remarks on the electric ſpark.

Theſe facts may ſerve to aſſiſt our meditations with regard to the nature of the electric ſpark. In a late paper by Mr. Biot, given at page 214 of our Vol. XII. the author makes an ingenious conjecture, that the light and heat in this phenomenon may have been produced by mechanical compreſſion

Whether Biot's theory of luminous condenſed air can be ſupported.

Warltire's fire-ball.

Combuſtion of flint and ſteel,

requires a very minute portion of metal:

Electric temperature is extremely elevated;

All metals loſe by electricity, and the ſpark paſſes only between combuſtible bodies.

Hence probably it was part of the body ſet on fire.

of the atmoſpheric air. Whether this ſuppoſition can be reconciled to the appearance of the ſpark in oil, and to ſome atmoſpheric phenomena, in which we are told of luminous balls moving apparently with little velocity through the air, and particularly that ſlowly-moving artificial fire-ball, produced once, and only once, by Warltire, as narrated in Prieſtley's work on air, may admit of queſtion. When we conſider that a particle of iron, cut off and ſet on fire in the common action of ſtriking a light, appears, from the vivacity of its combuſtion, to be a body of conſiderable magnitude, though the uſual quantity of metal would not form a ball of one thouſandth of an inch in diameter; when we conſider the prodigious elevation of temperature indicated by the exploſion of wires of all metals by the electric ſhock, particularly in thoſe beautiful and ſtriking experiments which Van Marum has publiſhed; and laſtly, when we call to mind that a metallic chain loſes part of its weight every time a ſhock is paſſed through it, and that the ſpark is never ſeen to paſs between incombuſtible bodies—conſiderable reaſons will preſent themſelves in favour of a modified ſuppoſition, that the electric ſpark may conſiſt of, or be accompanied by, a portion of the body from which it proceeds.

Fire-Balls, &c. may be electric ſparks;

Are not the atmoſpheric fire-balls or luminous meteors, the ſhooting ſtars and the ſtones which have fallen from the atmoſphere, electric ſparks upon a ſcale of immenſe magnitude?

and the ſpark a fire-ball.

If any luminous ball were to paſs with a ſwift angular motion over the field of view, it would have the appearance of a line or ſtreak of light. If it were to break in pieces many divergent ſtreaks would be ſeen. May not the electric bruſh be a phenomenon of this deſcription on a ſmall ſcale?

Facts are more wanted than conjectures.

It would not be difficult to apply this ſpeculation to the figures 1 and 2 before us; but as we are more in want of facts than of conjectures, and as it may be hoped that ſome of my readers who have the means and the time will purſue this inveſtigation, I ſhall for the preſent conclude.

SCIENTIFIC NEWS.

Anatomical Cabinet.

Anatomical cabinet.

THERE has appeared at Berlin, a complete deſcription of the anatomical cabinet of M. Walter, which the king has purchaſed, almoſt a year ago, for the ſum of 400,000 francs. This catalogue is compoſed of ſixty-two printed ſheets.

Shower of Peas.

Shower of peas.

Dr. Hiem, of Berlin, has publiſhed a note, in which he explains that the peas, which were ſaid to have fallen from the atmoſphere in a ſhower at Landſchut, in Sileſia, were merely tubercles which are ſeparated from the roots of ſeveral plants. Thoſe in queſtion, according to the Doctor, were afforded by the roots of the aquatic plant *Ranunculus Ficaria.* He pretends that an enormous maſs of theſe tubercules may have been formed in certain cavities, whence they might be carried to a diſtance by the whirl or eddy of ſtrong wind. He ſupports his opinion by the accounts of ſhowers of this nature given by the celebrated Klaproth in his Journal of Chemiſtry.— The Doctor concludes by remarking, that theſe tubercles contain a farinaceous ſubſtance equal in goodneſs to that of potatoes, and recommends an attention to the ficaria for this purpoſe.

Univerſal Language.

Univerſal language.

THE Celtic academy, in a ſitting of laſt April, made proof of a new diſcovery by one of its members; which gives the power of correſponding, and diſcourſing, with men, whoſe language is unknown, with expedition, without previous ſtudy, any expence, the leaſt trouble, or the ſmalleſt labour of the mind. The proof made at that ſitting by twenty-five academicians, on the languages of Europe, aſcertained, that by the aid of this invention, a man may travel any where without an interpreter, demand what he wants, diſcourſe on whatever ſubjects can intereſt any ſort of travellers, and even expreſs metaphyſical thoughts. It is intended to make this diſcovery public at the return of the Emperor.

The above account has appeared in ſeveral publications of credit, but it is probable the account is exaggerated in ſeveral reſpects.

Turkiſh Edict in Favour of Science.

Turkiſh edict in favour of ſcience.

THE Grand Seignor has conſtituted Prince Morouſi; by a diploma written with his own hand, director general of the hoſpitals of his empire, and inſpector of the ſchools of medicine, mathematics, and *belles lettres*, which his highneſs is engaged in founding with all poſſible diſpatch. This diploma is remarkable for the great praiſes of the ſciences made in it by the Grand Seignor; as they hitherto have been in no great favour with the Mahometans. In rendering juſtice to the ſkill of the Chriſtian phyſicians, who have ſtudied at the univerſities of Halle, of Padua, and of Montpelier, the Grand Seignour remarks with much truth, that theſe phyſicians, when brought into foreign countries, often commit great errors on account of the difference of the temperature of the climates; from whence he concludes that, in order to practiſe medicine well, it is neceſſary to ſtudy in the country where the profeſſion is to be exerciſed.

Coptic Manuſcripts.

Coptic manuſcript.

THE celebrated Daniſh antiquarian, M. Zoega, is daily occupied at Rome in completing his catalogue of Cophtic manuſcripts in the Borgheſe muſeum. He intends afterwards to publiſh a new topography of ancient Rome. It is probable this work will be printed in Germany, becauſe it will require numerous engravings, which no Italian bookſeller would chooſe to go to the expence of. It is, however, not believed that M. Zoega will occupy the profeſſor's chair, which has been granted him at the Univerſity of Kiel, as he is too much accuſtomed to the fine climate of Italy to leave it willingly.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1806.

ARTICLE I.

On the Cauſe of Fairy Rings. In a Letter from Mr. FLORIAN-JOLLY.

To Mr. NICHOLSON.

Aſſembly-houſe, Laytonſtone, Eſſex,
January 13.

SIR,

The phenomenon of fairy-rings

SEEING by the letter of Mr. Gough inſerted in the laſt Number of your Journal, that the cauſe of fairy-rings is not yet agreed upon among naturaliſts, I beg leave to ſubmit to their conſideration a few facts which I had occaſion to remark ſome years ago, during a ſummer reſidence in Hampſhire.

produced in great numbers in one diviſion of Broadlands park and none in other parts.

The park of Broadlands, Lord Palmerſton's ſeat, near Rumſey, was divided into three principal incloſures, formed by hurdles only. One of theſe had been lately mowed; there were cattle grazing in the next; and the other, which had afforded winter and ſpring fodder to ſome horſes kept at graſs, was then left to grow for an autumn crop. *This* laſt exhibited an immenſe number of fairy rings, ſome perfectly circular, ſome forming irregular curves, and others nothing but ſmall round

round patches: In all of theſe the graſs grew more luxuriant and of a deeper hue: No other fungus was to be found in any of them but the eſculent muſhroom. In the part lately mowed, and in that where the cattle were grazing, there was not the leaſt appearance of fairy-rings.

Another field abounding with them.

In the courſe of ſubſequent perambulations, I obſerved in a graſs field ſituated on the top of the firſt high ground upon the road from Rumſey to Saliſbury, appearances nearly ſimilar to thoſe exhibited in the growing graſs of the park. There had been all ſummer, and there were ſtill horſes grazing in this field: The fairy-rings were numerous, but the graſs in the rings and patches, inſtead of being more luxuriant, was completely dry and blaſted, and there grew two or three different fungi, all of them of thoſe ſorts which are reckoned noxious.

They were not produced by electricity,

That the fairy-rings at Broadlands were not the effect of electricity, appears to me beyond all doubt, ſince one part only of the park exhibited them, while the reſt of the contiguous grounds, divided from that part by nothing more than a row of hurdles, did not ſhew any ſuch appearance: otherwiſe it muſt be contended, that the electrical phenomena might take place on one ſide of the hurdles and never on the other, a predilection truly ſingular, and, I ſhould think, difficult to be accounted for.

but by the excrement of the horſes.

Another fact which I have repeatedly obſerved ſince that time, has led me to ſuſpect that the fairy-rings, their different appearances, and the various ſpecies of fungi found in them, might be produced by no more uncommon cauſe than the excrements of the horſes.

Argument from the appearances in hot-beds.

The hot-beds made of horſe-dung, which I have had ſeveral times in my garden, have generally produced in ſucceſſion the ſame fungi which are to be found in the different ſtates of the fairy-rings. Whilſt the beds are yet new, the fungi are of the ſame noxious ſpecies as I ſaw in the dry blaſted fairy-rings, but when they grow cooler and more matured, eſculent muſhrooms begin to grow naturally, and although no ſpawn was ever put in the bed.

I have alſo remarked, that horſe-dung produced in ſome ſeaſons an immenſe quantity of muſhrooms, and hardly any in others: This might perhaps be attributed to the different quality of the hay on which the horſes had fed; and this might explain why fairy-rings are to be found in ſome paſtures rather than in others.

Experiment with oil, to illuſtrate the deductions.

That fairy-rings ſhould be produced by the excrements of horſes, may be illuſtrated by a very ſimple fact, which it is in the power of every perſon to obſerve. If you let fall ſome oil upon a marble ſlab, or ſome other liquid upon ſome ſubſtance that will imbibe it, you will ſee it gradually ſpread round in a more or leſs regular form; ſometimes aſſuming the appearance of a patch, and frequently continuing to flow from the center to the circumference, where it accumulates in a much greater proportion than in the inner part of the circle, taking thus the form of a ring.

This accumulation of the fluid at the circumference may be eaſily explained. As the fluid expands, the preſſure from the center becomes gradually leſs, till at laſt there is no ſufficient force to overcome the reſiſtance oppoſed by the dry parts of the ſolid ſubſtance which has imbibed it: yet, in conſequence of the firſt impulſe, the fluid will continue to flow from the center through the ſmall channels already opened, and will thus accumulate in greater quantities at the boundaries where its expanſive motion is ſtopped.

Application.

The excrements of horſes, diluted by the rains and imbibed in the ſoil, muſt have an effect ſimilar to that juſt deſcribed. This effect muſt, beſides, greatly depend upon the nature of the ſoil and the facility with which it is pervaded by the fluid; hence the conſtant appearance of fairy-rings in ſome paſture-grounds, while none are ever to be found in others.

Should you, Sir, conſider theſe remarks, and the deductions which they have ſuggeſted to me, as likely to throw ſome light upon the cauſe of fairy-rings, you are welcome to make any uſe of them you may think proper.

I am, Sir,

Your obedient humble ſervant,

J. FLORIAN-JOLLY.

II.

Experiments on the Magnetiſm of ſlender Iron Wires. *By* JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR, *Middleſhaw, January* 9, 1806.

A general maxim in magnetiſm ſtated.

THE general phenomena of magnetiſm have given riſe to a maxim which ſhall be here ſtated in the words of a judicious writer on the ſubject. "The magnetiſm acquired by being placed within the influence or the ſphere of activity of a magnet in ſoft iron, laſts only while the iron continues in that ſituation; and when removed from the vicinity of the magnet, its magnetiſm vaniſhes immediately; but with hard iron, and eſpecially with ſteel, the caſe is quite different; for the harder the iron or ſteel is, the more permanent is the magnetiſm, which it acquires from the influence of a magnet." Cavallo on Magnetiſm, London, 1787, p. 30.

Remarks on this maxim.

This propoſition is of great utility in the ſcience, for it explains a variety of relations betwixt the magnet and ferruginous bodies, but I have obſerved one phenomenon that appears inexplicable on the principle, and conſequently may be ſaid to offer one exception to the general propoſition. As my experiments on the ſubject are very eaſy, it ſeems adviſeable to deliver the leading circumſtances in the form of ſo many precepts becauſe this method will aſſiſt any one deſirous of purſuing the enquiry, to repeat them with eaſe.

An experiment conſiſtent with the maxim.

Experiment 1. Apply either pole of a ſtrong magnet to one end of a ſhort horizontal bar of clean ſoft iron, and a particle of iron equally ſoft to the other end. This particle will remain ſuſpended at the extremity of the bar until the magnet is withdrawn; but the removal of this power will diſſolve the connection ſubſiſting betwixt the two pieces of iron, and the particle will drop off immediately.

An experiment contradicting the maxim.

Exp. 2. The preceding experiment confirms the maxim ſtated above, when conducted according to the foregoing directions; but let it be repeated with the following alteration, and it will contradict the general propoſition. In place of the particle of ſoft iron, ſubſtitute a piece of iron wire of number 39 in the wire drawers ſcale, the weight of which may amount to two or three grains. The removal of the magnet will

[illegible]

Fig 1

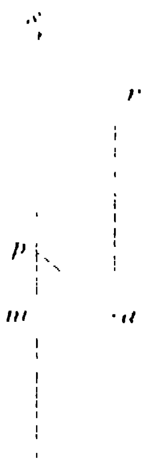


Fig. 2

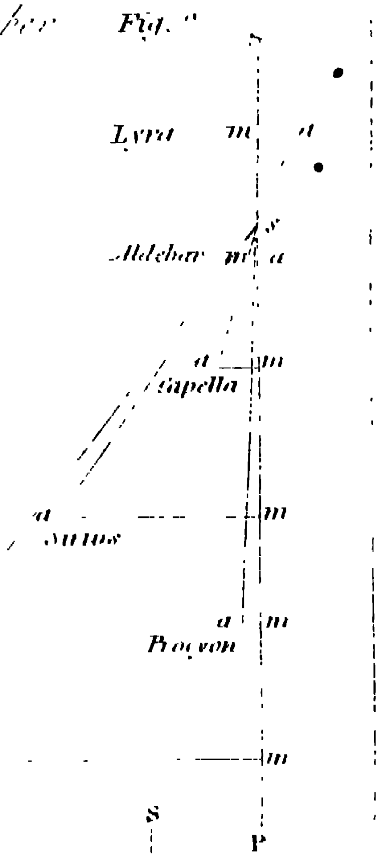


Fig 3

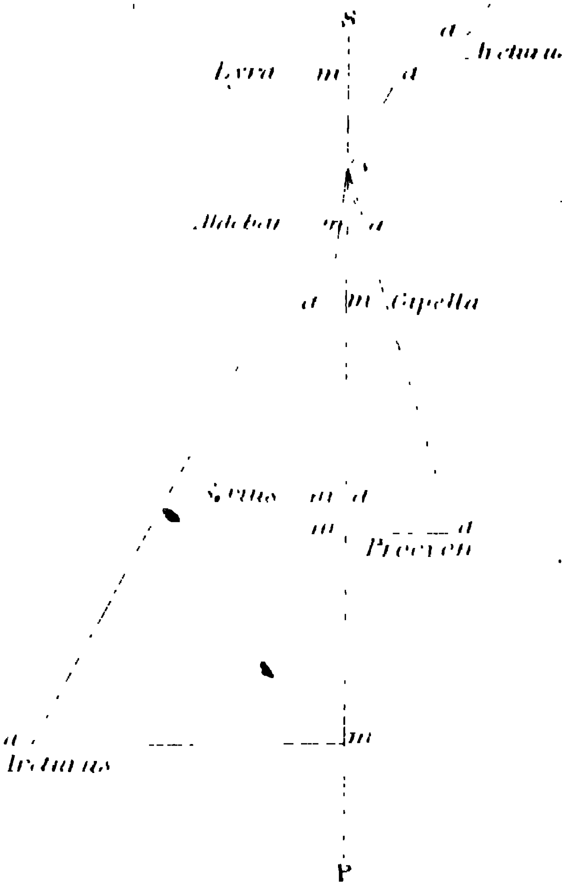
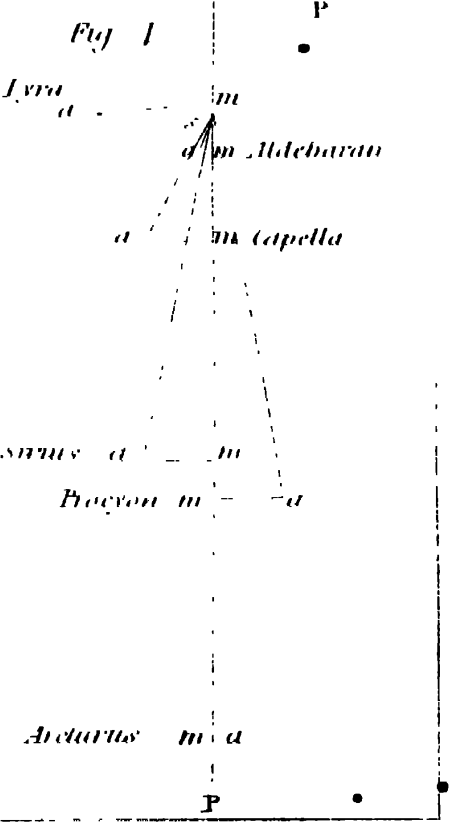


Fig 4



will not break the connection formed by its preſence between the bar of ſoft iron and the wire; for the latter will remain attached to the end of the former, by the extremity which was firſt brought into contact with the iron; if the piece of wire be removed from the end of the bar, the magnetic connection may be revived by replacing it immediately. The ſame thing will happen if the wire be expeditiouſly transferred from the firſt bar to another rod of ſoft iron; but it loſes its magnetiſm in the ſpace of two or three ſeconds when kept at a diſtance from all ferruginous bodies which are capable of attracting it, and of being attracted by it. Theſe facts prove wire of number 32 to be a magnet, the virtue of which is conditional, becauſe its permanency depends on the preſence of ſoft iron, and perhaps on no other circumſtance; for the experiment may be repeated with ſucceſs upon ruſty wire of the ſame ſize, or on pieces which have been made red hot in the flame of a candle, or ſurrounded by ſand in a crucible, in which ſituation they will cool much more gradually than when drawn ſingly through a flame.

Wires of low ſizes not conditional magnet.

Exp. 3. This capacity of iron wire to preſerve the magnetiſm imparted to it, as long as it remains in contact with a bar of the ſame metal, is a property confined to certain ſizes; for let the firſt experiment be repeated with a ſmall piece of numbers 18 or 17, not equal to half a grain in weight, and juſt as it comes from the hand of the workman, this piece will perform the part of a particle of ſoft hammered iron, that is it will drop from the end of the bar, to which it has been attached by the application of a magnet, to the oppoſite extremity, as ſoon as the magnetic influence ceaſes to act upon it; conſequently the mere operation of drawing ſoft iron into wire, by forcing it through a conical hole too narrow for its preſent diameter, will not convert it into a conditional magnet.

The loweſt ſize capable of conditional magnetiſm aſcertained.

Amongſt other experiments relating to the ſubject, I took the trouble to examine the quality of every ſize from 32 to 21, both incluſive; the 11 ſmalleſt wires, the extreams of which were 32 and 22, were all conditional magnets; that is, they all adhered to the bar of ſoft iron, to which they had been previouſly attached, after the removal of the magnet. Number 23 ſupported ſeven grains of lead including its own weight, without the aſſiſtance of the magnet; No. 24, 6½ nearly; No. 32, 4½; No. 22, no more than two grains.

As for number 21, it poſſeſſed the ſimple properties of ſoft iron: for the ſhorteſt cylinder which could be taken from a rod of this ſize by means of a cutting file, dropped from the end of the horizontal bar as ſoon as the magnet was withdrawn.

Remarks on Exp. 3d.

It is difficult to ſay, which of the 11 wires mentioned above, had the magnetic virtue in the moſt perfection, becauſe each piece differed in diameter from the reſt; beſides which, it is very well known, that a maſs of iron, of a weight and figure determinable by experiment only, is attracted by any particular magnet, more powerfully than any other maſs of the ſame metal. But the preceding trials have diſcovered one circumſtance apparently of ſome importance, for they ſhew that wire is converted into a conditional magnet by its paſſage through the 22 wordle, or wire drawers inſtrument; and that the 23d operation brings this quality in it to perfection as far as we can judge from experiment.

I here only ſpeak of wire drawn in Kendal, for I have been told, that the ſame article manufactured in ſome parts of Yorkſhire, has a much greater propenſity to become magnetical. This information was communicated to me by Mr. Morrice, a very intelligent ſuperintendant of a manufacture of cards in this town; who moreover obſerved, that wire of this deſcription acquires a degree of magnetiſm under the ſhears, which induced him, when employed in working it, to ſubſtitute a braſs gauge for the common inſtrument made of iron.

Conjectures relative to the cauſe.

The magnetic property which commences with number 22, ſeems to be common to all the finer ſizes, for I found it in the ſmalleſt wire I could procure, and which apparently did not exceed a ſtrong human hair in thickneſs.

The foregoing experiments, beſides proving that ſlender wires acquire a magnetiſm which is permanent as long as they remain in contact with iron, alſo affords an exception to a ſecond general maxim of the ſcience, which aſſerts, that the permanency of communicated magnetiſm depends on the hardneſs of the ferruginous body that receives it. This does not appear to be the caſe in experiment 2, in which wire of No. 32 did not loſe the faculty of being convertible into a conditional magnet after undergoing a red heat, a proceſs that is well known to render wire very ſoft. I even repeated the experiment with the ſame reſult on all ſizes betwixt 22 and 33, except 26; pieces of each ſort were heated both in the flame of a candle, and in ſand; all of which retained the faculty under conſideration after being treated in both ways. In reality, wires that had been thus ſoftened, ſeemed to be in the ſame condition with ſmall nails of caſt iron, conſidered as retainers of magnetiſm, though the latter are of a much harder quality; for a nail of the ſort called ſparrow-bills by ſhoe-makers exhibited the appearances deſcribed in the ſecond experiment, after being filed down to the thickneſs of a ſmall wire.

If then that kind of magnetiſm which I have ventured to call conditional do not depend on comparative hardneſs, to what cauſe is the phenomenon to be deſcribed? little can be offered on my part, beſides probable conjecture, in anſwer to this queſtion. The temperature of wire is conſiderably raiſed during its paſſage through the wordle; and may not we imagine with ſome ſhew of reaſon, that this encreaſe of temperature, joined to the ſubſequent contact of cold air, produces a new arrangement of the molecules conſtituting the wire which enables it to retain a portion of magnetiſm as long as it remains in contact with a ferruginous body? if this ſuppoſition be true, experiment proves the new arrangement o take place in the 22 wordle; when the ſlenderneſs of the wire will occaſion it to cool ſuddenly after paſſing through the nſtrument. The reality of ſuch changes in the texture of bodies which are not in a ſtate of fuſion, is admitted at preſent by experimental philoſophers. I may alſo quote in favour of this hypotheſis ſome valuable obſervations made by Gregory Watt, Eſq, on the various degrees of magnetiſm exhibited by the ſame bazaltic ſtone under different forms of cryſtallization; which obſervations may be ſeen in your Journal for February, 1805.

Any attempt to explain the permanent magnetiſm of ſmall wires during their connection with ſoft iron, and the loſs of his property which enſues when the connection is broken, appears to be ſuperfluous, becauſe the fact is evidently analogous to the well known method of adding ſtrength to a nagnet by a gradual encreaſe of its load; for this operation, when judiciouſly conducted, gives a magnetic charge to a bar of ſteel already touched, which it cannot retain after the weight is removed.

I remain, &c.

JOHN GOUGH.

P. S. I neg-

P. S. I neglected to mention the following circumſtance in the body of the letter. The drawing inſtrument, or wordle, is made of ſteel; and is it not probable that this tool, poſſeſſing a ſlight degree of magnetiſm given to it by friction or otherwiſe, aſſiſts in producing the neceſſary arrangement, by acting upon heated and ſlender wires, while their molecules are in a violent motion from the preſſure of the inſtrument itſelf? This ſuppoſition has ſome claim to plauſibility; becauſe a weak magnet will impart a portion of the ſame virtue to a bar of tempered ſteel, the particles of which are in a ſtate of vibration; for a rod of this metal will acquire a degree of polarity, provided it be ſtruck on the end with a hammer when its axis lies parallel to the dipping-needle.

III.

Concerning the Differences in the magnetic Needle, on Board the Inveſtigator, ariſing from an Alteration in the Direction of the Ship's Head. By MATTHEW FLINDERS, *Eſq. Commander of his Majeſty's Ship Inveſtigator. From the Philoſophical Tranſactions,* 1805.

The magnetic needle is affected at ſea by the poſition of the ſhip's head:

WHILST ſurveying along the ſouth coaſt of New Holland, in 1801 and 1802, I obſerved a conſiderable difference in the direction of the magnetic needle, when there was no other apparent cauſe for it than that of the ſhip's head being in a different direction. This occaſioned much perplexity in laying down the bearings, and in allowing a proper variation upon them, and put me under the neceſſity of endeavouring to find out ſome method of correcting or allowing for theſe differences; for unleſs this could be done, many errors muſt unavoidably get admiſſion into the chart. I firſt removed two guns into the hold, which had ſtood near the compaſſes, and afterwards fixed the ſurveying compaſs exactly a-midſhips upon the binnacle, for at firſt it was occaſionally ſhifted to the weather ſide as the ſhip went about; but neither of theſe two arrangements produced any material effect in remedying the diſagreements.

ſcarcely from the iron on deck.

The following table contains the obſervations for the variation of the compaſs in which the differences are moſt remarkable, and from which I ſhall beg to point out ſuch inferences as I think may be drawn from them.

TABLE, *ſhewing the Errors produced in the Magnetic Compaſs by the proper Magnetiſm of the Ship.*

Time.	Latitude.	Longitude.	Number of compaſſes uſed.	Number of ſets of obſervations taken.	Place of the compaſs.	Suppoſed true variation.	Obſerved variation.	Ship's head.	Obſerver.
1801.									
Dec. AM	35° 5' S	116° 28′ E	two	4 azimuths	binnacle	7° 0' W	5° 59′ W	NW b. N	Commander
	Princeſs Royal Harbour		three	6 —	on ſhore	6 15	6 23	—	
	—	—	theodolite	1 —	—	—	6 8	—	
1802.									
Jan. 9, PM	34 1	121 20	one	2 —	binnacle	5 0	9 22	ESE	
16, AM	Gooſe Iſland Bay		one	2 —	—	4 0	0 54	W	
1803.									
May 20, AM	—	—	—	2 —	—	—	6 8	E	
1802.									
Jan. 18, PM	33 37	124 10	—	2 —	—	4 30	5 44	NNE	
20, PM	32 33	125 35	—	2 —	—	—	7 15	E b. N	
21, PM	32 30	125 48	—	2 —	—	—	4 45	S	
22, PM	32 30	126 7	—	2 —	—	4 15	6 13	NE b. E	
23, PM	32 21	126 33	—	1 amplitude	—	4 0	4 18	S b. E	
24, AM	32 5	128 15	—	2 azimuths	—	3 0	6 4	E b. N	
26, PM	32 15	128 15	three	6 —	—	—	3 7	S b. E	Lt. Flinders
30, AM	32 18	132 29	one	2 —	—	0 30	1 41	SSE	Commander
Feb. 4, AM	No. 4, bay in iſland e		—	2 —	—	0 15	2 23	Eaſterly	
5, AM	32 39	133 55	—	1 amplitude	—	—	1 56	E b. S	
6, AM	32 36	133 58	three	6 azimuths	—	—	1 0 E	NW	Lt. Flinders
6, PM	34 3	135 20	one	2 —	—	1 5 E	1 33 W	SE b. E	Commander
—, PM	34 5	135 24	—	1 amplitude	—	1 5	3 56 E	SW	

TABLE, ſhewing the Errors produced in the Magnetic Compaſs by the proper Magnetiſm of the Ship.

Time.	Latitude.	Longitude.	Number of compaſſes uſed.	Number of ſets of obſervations taken.	Place of the compaſs.	Suppoſed true variation.	Obſerved variation.	Ship's head.	Obſerver.
1802.									
Feb. 18, AM	34° 50′ S	135° 32′ E	three	6 azimuths	binnacle	1° 12′ E	1° 12′ E	S	Lt. Flinders
Mar. 1, PM	In No. 10, bay		theodolite	1 —	on ſhore	1 39	1 39	—	Commander
5, PM	—	—	one	2 —	binnacle	1 39	0 53	S b. E	
17, PM	34 12	137 20	—	1 amplitude	—	2 15	4 38	SW b. S	Commander
18, PM	34 23	137 36	one	2 azimuths	binnacle	2 15	0 35	SE	
21, AM	35 33	137 15	three	6 —	—	2 40 E	1 10	SE b. S	
23, AM	Kanguroo Iſland		two	4 —	—	2 58	6 31	SSW	
26, AM	35 10	137 41	one	1 amplitude	—	2 45	1 49	NE b. N	
27, AM	35 21	137 52	one	1 —	—	2 50	1 49	SSE	
April 6, AM	Kanguroo Iſland		—	2 azimuths	on ſhore	2 58	2 58	—	
10, AM	35 47	139 15	—	1 amplitude	binnacle	3 0	5 11	W b. S	
11, PM	35 53	139 26	—	2 azimuths	—	3 0	0 50	SE	
13, PM	36 45	140 5	—	2 —	—	3 30	1 25	SE b. S	
16, AM	37 55	139 55	—	2 — 1 amplitude	—	4 15	2 20	—	
17, PM	37 57	139 56	—	2 azimuths	—	4 15	2 2	NE	
22, AM	39 38	144 50	—	2 —	—	7 45	11 52	WSW	
26, PM	38 35	144 25	—	2 —	—	7 30	3 41	NE b. E	
—, AM	38 38	144 35	—	1 amplitude	—	7 30	6 48	NE b. N	

Note. All the compaſſes made uſe of on board the Inveſtigator were of Walker's conſtruction, one excepted, which was made by Adams, and uſed only on July 22, 1801.

It is apparent that ſome of the obſerved variations in the above table are 4° leſs and others 4° greater than the truth; and it may be remarked that when this error is weſtward, the ſhip's head was eaſt or nearly ſo, and when it was eaſtward the head was in the oppoſite direction. When the obſervations agree neareſt with what was taken on ſhore, or with what may be deemed the true variations the ſhip's head was nearly north or ſouth; and a minute inſpection of the table will favour the opinion, that the exceſs or diminution of the variation was generally in proportion as the ſhip's head inclined on either ſide from the magnetic meridian.

The errors were about 4° each way, and the north end of the needle deviated as if repelled by the ſhip's head, &c.

After I had well aſcertained the certainty of a difference in the compaſſes, ariſing from an alteration in the point ſteered, I judged it neceſſary, when I wanted a ſet of bearings from a point where we tacked the ſhip, to take one ſet juſt before and another immediately after that operation: ſome ſpecimens of theſe here follow.

Other obſervations.

1802.		*Head* ESE.		*Head* SW b. W.
April 13th, $11^h 32'$ AM	Le Geographe Rocks	N 55° to 71° E		
	Ξ point	N 4 W	after tacking	N 9° W
—	Π point	S 32 E	-	S 40 E.
		Head SE b. E.		*Head* W.
April 14th, $9^h 29'$ AM	Π point rocky, inner part	N 39° E.	after tacking	N 30° E
	——— projecting part	N 67 E	-	N 59 E
—	Furtheſt viſible extreme from deck	S 51 E	-	S 55 E.
		Head ENE.		*Head* SW b. S.
April 15th, $11^h 50'$ AM	N, the weſtern part	N 15° W.	after tacking	N 21° W
	A peaked hummock	N 19 E	-	N 15 E
—	Furtheſt extreme from deck	S 53 E	-	S 61 E
—	Centre of a naked ſandy patch	E	-	E 5 N.

Variation per amplitude April 15, AM, taken with the ſurveying compaſs } 4° 8′ E, ſhip's head being S.

		Head E.	*Head* SW b. S.
April 15th, 5^h PM	The peaked hummock	N 12° W	after tacking N 18° W
	Former extreme, a projection	S 59 E	S 64 E
—	Naked ſandy patch, diſtant 3½ -	N 33 E	N 31 E.

Limits of error in obſerving bearings on ſhip-board.

They may amount to two or even three degrees.

From ſome little change of place after tacking the ſhip, and from the part whoſe bearing was ſet not being perhaps the individual ſpot in both inſtances, the difference between the ſeparate bearings in any ſet will not be always the ſame: to theſe cauſes for error alſo may be added inacurracies in taking the angles ariſing from the motion of the ſhip and compaſs, from the view of the object being obſtructed by the rigging, maſts, or ſhip's upper works, and from too much haſte to get the bearings before the ſhip's place was materially altered. Even in the Table of azimuths and amplitudes greater accuracy than one degree muſt not be looked for; and in ſhip-bearings two or even three degrees is not, I believe, too great an allowance for error, unleſs in very favourable circumſtances.

Reſults ſimilar to thoſe firſt ſtated.

Without attending to ſmall differences, it is evident that the bearings correſpond with the obſervation in requiring a leſs eaſt variation to be applied when the ſhip's head was eaſterly, and a greater when it was to the weſtward, in order to get at the true direction of the object *. When examining the north and

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* As a ſpecimen of the plan I followed in protracting ſuch bearings as the above, take the ſet of April 15, A M, when the true variation appears to have been 4° E. On the firſt bearing the ſhip's head was ſix points on one ſide of the meridian, and on the ſecond it was three points on the other ſide, the mean is one point and an half on the eaſt ſide; now for this one point and an half I allow 1° of error, which, as it is on the eaſt ſide of the meridian, and the variation is eaſterly, muſt be ſubtracted: the variation then to be allowed upon the mean between the bearings before and after tacking will be 3° E, from which the true bearings will ſtand as follows:

April

and eaſt coaſts of New Holland, I always endeavoured to take the angles on ſhore with a Troughton's portable theodolite, and to obſerve for the variation in the ſame places, that all the errors might be done away or corrected; and as I was frequently fortunate enough to carry on my ſurveys in this manner for weeks together, inſtances that might corroborate or contradict the preceding remarks are neither very numerous or pointed; the following are the moſt remarkable:

April 15th, AM 11h 50'	Π weſtern part - -	N 15° E
	A peaked hummock - -	N 20 E
—	Furtheſt extreme from deck -	S 54 E
—	Centre of a naked ſandy patch -	E 0½ S.

In the ſame manner upon ſingle ſets of bearings I was obliged to allow a variation different from what I ſuppoſed the true to be, unleſs the ſhip's head was nearly north or ſouth: but, that I might proceed as little upon conjecture as poſſible, I always endeavoured to get obſervations for the variation when the ſhip's head was in the ſame direction as when I had taken or wiſhed to take a particular ſet of bearings, and I then allowed that variation exactly, whatever it was. The perplexity ariſing from diſagreements in bearings was by theſe means much alleviated, and happy agreements were frequently produced, when, without ſuch corrections, there was nothing but diſcord.

TABLE of obſerved Variations of the Compaſs, and of the Influence of the Ship's Poſition upon them.

Time.	Latitude.	Longitude.	Number of compaſſes uſed.	Number of ſets of obſervations taken.	Place of the compaſs.	Suppoſed true variation.	Obſerved variation.	Ship's head.	Obſerver.
1802.									
Aug. 5, PM	23° 51′ S	151° 42′ E	one	1 amplitude	binnacle	8° 0′ E	12° 7′ E	WSW	Commander
— AM	23 51	151 40	—	---	—	—	10 15	WNW	
12, PM	23 30	151 11	three	6 azimuths	—	—	6 50	SSE	Lt. Flinders
18, PM	23 25	151 16	one	2 —	—	7 45	7 52	W	Commander
31	22 23	150 38	two	4 —	—	7 30	4 49	E	
Sept. 6, AM	Upon Pier Head		theodolite	1 —	on ſhore	8 0	8 2	—	
Oct. 14, PM	20 44	150 42	one	1 amplitude	binnacle	7 0	6 40	SSE	Lt. Flinders
20, PM	19 22	148 40	—	1 —	—	6 0	5 39	S	Commander
21, AM	18 15	148 38	three	6 azimuths	—	—	5 42	N b. E	Lt. Flinders
Nov. 2, PM	10 30	142 32	one	2 —	—	4 0	3 32	E	Commander
7, AM	12 11	142 0	—	2 —	—	—	4 4	S	Lt. Flinders
9, PM	12 37	142 2	—	1 amplitude	—	—	5 24	W	Commander
1803.									
Jan. 3, PM	14 20	136 16	—	1 —	—	2 30	0 58	E	
7, PM	14 20	136 37	—	1 —	—	—	1 9	SE	
13, PM AM	13 38	137 20	—	2 —	—	3 0	3 47	Weſterly	Lt. Flinders
14, AM	13 35	136 58	—	1 —	—	—	5 51	WSW	Commander
16, PM	In NW Bay (Gr. Eyl.)		theodolite	1 azimuth	on ſhore	—	3 6	—	
Feb. 3, AM	Arnhem S Bay		three	6 —	binnacle	2 20	2 26	NW b. W	
9, AM	—	—	theodolite	1 —	on ſhore	—	2 20	—	
Mar. 10, AM	11 5	134 15	one	2 —	binnacle	1 0	1 55	WNW	

In the latter of theſe obſervations, the differences ariſing from a change in the direction of the ſhip's head is leſs conſiderable than in the higher latitudes; indeed, on approaching the line of no variation upon the ſouth coaſt, the differences in the variation were ſmaller than before and afterwards; but that theſe differences ſhall be greater in a large variation and ſmaller in a leſs, both places being equally diſtant from the magnetic pole, I will not venture to aſſert. The inferences that I think may be ſafely drawn from the above obſervations are as follows: 1ſt. That there was a difference in the direction of the magnetic needle on board the Inveſtigator when the ſhip's head pointed to the eaſt, and when it was directed weſtward. 2d. That this difference was eaſterly when the ſhip's head was pointed to the weſt, and weſterly when it was eaſt. 3d. That when the ſhip's head was north or ſouth the needle took the ſame direction or nearly ſo that it would on ſhore; and ſhewed a variation from the true meridian, which was nearly the medium between what it ſhowed when eaſt and when weſt. 4th. That the error in variation was nearly proportionate to the number of points which the ſhip's head was from the north or ſouth. Conſtant employment upon practice has not allowed me to become much acquainted with theories, but the little information I have upon the ſubject of magnetiſm has led me to form ſome notion concerning the cauſe of theſe differences, and although moſt probably vague and unſcientific, I truſt for the candour of the learned in ſubmitting it, as well as the inferences above drawn, to their judgment.

General inferences. 1. The compaſs was found to deviate accordingly as the ſhip's head was eaſterly or weſterly; the north end being farther from the ſhip's head than the truth. The deviation was proportional to the diſtance of the head from the N. or S.

1ſt. I ſuppoſe the attractive power of the different bodies in a ſhip, which are capable of affecting the compaſs, to be collected into ſomething like a focal point or center of gravity, and that this point is nearly in the center of the ſhip where the ſhot are depoſited, for here the greateſt quantity of iron is collected together.

2d. I ſuppoſe this point to be endued with the ſame kind of attraction as the pole of the hemiſphere where the ſhip is; conſequently, in New Holland the ſouth end of the needle would be attracted by it and the north end repelled.

Theory propoſed. That all the iron in the ſhip acts like one magnet, having a different polarity accordingly as the ſhip is near the N. or S. pole of the terreſtrial magnetiſm.

3d. That the attractive power of this point is ſufficiently ſtrong in a ſhip of war to interfere with the action of the magnetic poles upon a compaſs placed upon or in the binnacle.

Inferences from the laſt ſuppoſition:

that the effects ſhould have a contrary direction in north latitude.

If theſe ſuppoſitions are conſiſtent with the laws of magnetiſm, eſtabliſhed by experiments, I judge that they will account for all the differences above noticed; for the interference will neceſſarily be moſt perceptible upon a compaſs when the attractive point is at right angles to the magnetic meridian, that is, when the ſhip's head is eaſt or weſt, and will altogether vaniſh or become imperceptible when the attractive point and meridian coincide, or when the ſhip's head is north or ſouth. That the power of this point ſhould become leſs as the ſhip increaſes her diſtance from the magnetic pole has not indeed entered into my ſuppoſitions; but it may probably be true, and is indeed almoſt a neceſſary conſequence of the ſecond ſuppoſition. If the above hypotheſis, ſo to call it, be true, it muſt follow, that the differences i nthe variation of the magnetic needle, ariſing from a change in the ſhip's head, ought to be directly contrary to thoſe before recited, when the ſhip is on the north ſide of the magnetic equator, for the north point of the needle ſhould then be attracted, and the ſouth end repelled. I have no obſervations which are very deciſive upon this head, but thoſe that were taken on board the Inveſtigator ſeem to beſpeak that as it is ſo; they are as follow.

TABLE of Obſervations to illuſtrate the foregoing Inferences.

Time.	Latitude.	Longitude.	Number of compaſſes uſed.	Number of ſets of obſervations taken.	Place of the compaſs.	Suppoſed true variation.	Obſerved variation.	Ship's head.	Obſerver.
1801. July 21, PM	Start Point in ſight to the NE.		two	5 azimuths	binnacle	—	29° 34′ W	W	Mr. Thiſtle
—			one	1 amplitude	—	—	29 30	—	—
22, PM	49° 10′ N	5° 25′ W	two	4 azimuths	upon the booms in the middle of the ſhip	—	24 12	WNW	—
—, AM	48 15	6 45	one	1 amplitude		—	24 49	WSW	—
28, PM	38 1	14 20	five	10 azimuths		—	20 57	SW	—
—	—	—	—	11 —	binnacle	—	25 34	—	Commander
31, PM	Porto Santo in ſight to the NW		two	4 —	—	—	22 45	—	Mr. Thiſtle
—			—	4 —	booms	—	19 51	—	—
Aug. 24, AM	10 20	22 15	one	2 —	binnacle	—	12 45	SE b. S	Commander
29, AM	5 40	16 30	two	4 —	—	—	12 18	—	Lt. Flinders
Sept. 5, AM	2 15	14 00	—	3 —	—	—	14 54	WSW	Mr. Thiſtle

Theſe obſervations, particularly thoſe of July 28, ſeem to be deciſive in ſhowing that the variation is more weſterly when taken upon the binnacle of a ſhip whoſe head is weſtward in north latitude, than when obſerved in the center of the ſhip, which is a ſtrong confirmation of the ſuppoſitions before given; but the obſervations on the change of the ſhip's head are too few to be ſatisfactory. Almoſt every ſea officer can tell whether he has obſerved the variation of the compaſs to be greater when going down the Engliſh Channel than when coming up it: and indeed it would be very eaſy for a ſhip lying in harbour to aſcertain the point beyond controverſy.

Remarks and obſervations on the ſame law.

Should this point be well eſtabliſhed, I think it would follow, that from a high ſouth latitude where the differences are great on one ſide, they are moſt likely to decreaſe gradually to the equator, and to increaſe in the ſame way to a high north latitude, where they are great on the other ſide; thus the ſmaller differences on the north coaſt of New Holland will be accounted for. I ſhall leave it to the learned on the ſubject of magnetiſm to compare the obſervations here given with thoſe made by others in different parts of the earth, and to form from them an hypotheſis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profeſs to underſtand the ſubject.

Inſtances of the compaſs being affected by local magnetiſm.

Some account of the magnetiſm of Pier Head, upon the eaſt coaſt of New Holland, may not perhaps be thought an unappropriate concluſion to this Paper. I was induced to attend to this from the following paſſage in Hawkeſworth, Vol. III. p. 125. "At ſun-riſe I went aſhore," ſays Captain Cook, "and climbing a conſiderable hill," Pier Head, "I took a view of the coaſt and the iſlands that lie off it, with their bearings having an azimuth compaſs with me for that purpoſe; but I obſerved that the needle differed very conſiderably in its poſition, even to thirty degrees, in ſome places more, in others leſs; and once I found it differ from itſelf no leſs than two points in the diſtance of fourteen feet *. I took up ſome

Pier Head.

Local deviation of the compaſs as far as 50°.

* In a ſet of angles taken near the head of Arnhem north bay, on the weſt ſide of the gulph of Carpentaria, I found the needle of the theodolite had been drawn 50° from its proper direction. The ſhore conſiſted of grains of iron ore caked into a ſtony maſs; and a piece of it, when applied to the needle, drew it ſix or eight degrees

ſome of the looſe ſtones that lay upon the ground, and applied them to the needle, but they produced no effect; and I therefore concluded that there was iron ore in the hills, of which I had remarked other indications, both here and in the neighbouring parts."

The author's obſ. at Pier Head.

On landing at Pier Head I found the ſtones lying on the ſurface to be porphyry, of a dark bluiſh colour; but although I underſtand this ſpecies is uſually found to poſſeſs ſome magnetic power, a piece did not produce any ſenſible effect upon the needle of the theodolite when applied to it. In the following obſervations the theodolite always ſtood about four feet from the ground, that being nearly the length of its legs. I firſt took an extenſive ſet of bearings from the top of the hill, amongſt which were two ſtations whence Pier Head had been before ſet. The firſt, called Extenſive Mount, diſtant 34 miles, differed from its back bearing 4° 35′ to the right, and the ſecond, iſland *a*, diſtant 29½ miles, differed 4° 45′ the ſame way. I now moved the theodolite three yards to the weſtward, and the ſame two objects bore 2° 10′ to the right of their back bearing; on moving it three yards to the ſouth-eaſtward from the firſt place, they differed 2° to the left; and on moving the theodolite four yards to the northward, the ſame two objects bore 1° 10′ to the right of their back bearings. On the following morning I determined to try the magnetiſm more particularly. Taking the theodolite and dipping-needle, I landed upon the ſhore of the Head, whence the top of the hill bore N 50° W, about one-third of a mile. The variation of the

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degrees from its direction, but it then ſwung back to its error of 50° where it was ſtationary. In Arnhem ſouth bay a ſmall piece of ſimilar ſtone drew the needle of the theodolite entirely round, yet the bearings taken in this place did not ſhow any diſagreement from the variation and bearings taken in the neighbouring places, where the ſtone did not produce any ſuch effect. In moſt places on ſhore, where I had occaſion to take angles, it was my practice to try the effect of a piece of the ſtone upon the theodolite, in order to detect the preſence of iron ore, as well as on account of my ſurvey. It commonly happened that no effect was apparent, but yet I could not truſt implicitly to the angles, (particularly on the main land,) unleſs obſervations for the variation were taken before the inſtrument was moved, or I had a back bearing of ſome ſtation where ſuch obſervations had been made.

The author's obſ. at Pier Head.

theodolite in this place I obſerved to be 8° 2′ E, and the inclination of the ſouth end of the dipping needle 50° 50′, the needle ſtood vertical when the face of the inſtrument was S 2° E. I then took the following bearings: Extenſive Mount 108° 30′, the ſame exactly as by back bearing. Double Peak 143° 30′; from hence I rowed round the Head, and landed on a rock, whence the top of the hill bore SSW one-ſixth of a mile; Extenſive Mount bore 110° 14′, the inclination of the dipping-needle 50° 29′, and the needle ſtood vertical when the inſtrument faced S 3° E. Thus the difference was 1¼° in the horizontal, and ½° in the vertical direction of the needle. Aſcending the hill, I made the following obſervations on the top: Extenſive Mount 113° 50′, *a* iſland 133° 52′, Double Peak 148° 32′; the inclination of the needle was 53° 20′, and is ſtood vertical at S 3° E. The differences here are 5° 10′ in the horizontal, and 2° 30′ in the vertical direction, from what the needle ſtood at in the firſt morning's place. On moving ten yards SSE, the bearings were, Extenſive Mount 108° 44′, Double Peak 143° 25′; the inclination was 52° 18′, and the needle was vertical when the inſtrument faced S 5° W. In this 4th ſet of obſervations, the horizontal direction of the needle is only a few minutes different from the firſt place, but the vertical direction is 1° 28′. From the top of the hill I now moved twenty yards to the north-eaſtward,, when Extenſive Mount bore 110°, Double Peak 144° 42′; the inclination of the dipping needle was now 50° 35′, and it ſtood vertical at S 3° W. Thus it appears that the polarity of the magnetic needle is moſt interrupted at the top of the hill, both according to the theodolite and dipping-needle. Whether this may ariſe from ſome particular magnetic ſubſtance lodged in the heart of the hill, or from the attractive powers of all the ſubſtances which compoſe Pier Head being centered in a ſimilar point to what I have ſuppoſed to take place with all the ferruginous bodies lodged within a ſhip, I ſhall not attempt to decide. The greater differences in the horizontal direction of the needle obſerved by Captain Cook, might have ariſen from his uſing a common azimuth compaſs, which was probably not further elevated from the ground than to be placed on a ſtone.

MATTHEW FLINDERS.

Iſle of France,
March 5th, 1804.

Letter

IV.

Letter from Mr. Robert Harrup, *ſhewing that the Smut in Wheat exiſts in the Seed, and is greatly remedied by Lime ſteeping.*

To Mr. NICHOLSON,

SIR, *Chobham, January* 7, 1806.

LITTLE converſant in agricultural affairs, I am yet to learn what enquiries have been made into the nature and cauſes of the diſeaſes of grain.

If the following communication on the diſeaſe of wheat, known by the name of *ſmut*, contains any thing new, or may lead to farther inveſtigation; an early inſertion of it will greatly oblige,

Sir,

Your obedient humble Servant,

R. HARRUP.

Different cauſes have been aſſigned for the production of ſmut; ſome ſuppoſing it to ariſe from too great an abundance of water ſhoots, others from intemperate ſeaſons.

Cauſes uſually aſſigned for the ſmut.

A writer in a reſpectable publication ſtrenuouſly contends in favour of the latter opinion.

He informs us, that brine, pickling, liming, change of ſeed, and ſeed of one year old, and upwards, avail nothing. In cold wet ſummers, ſays he, the ſmut prevails notwithſtanding the uſe of every means which invention hath urged or ingenuity practiced. After a number of obſervations, he continues, "to ſum up the whole of this matter, it ſeems as certain as demonſtration can render it, that the ſmut is not owing to any defect or imperfection in the ſeed, but entirely to ſome corrupt-creating principle in the atmoſphere, in the blowing ſeaſon, which blights and deſtroys the grain in ſome ſhape or other, according to the time it has been blowing, when it is ſtruck with the blight."

Intemperate weather: and that attention to the ſeed avails nothing.

On the contrary, it would appear from the accounts of thoſe who have the moſt frequent opportunities of making obſervations, that the primary cauſe of ſmut is in the ſeed. All the farmers I have converſed with on the ſubject, are

Practical men uſually aſcribe it to the ſeed, and uſe preparations.

decidedly of opinion, that ſmut in the ſeed will produce ſmut in the crop, unleſs certain means are uſed to prevent it. With this intention I have ſomewhere ſeen a variety of preparations recommended, in ſome of which arſenic was one of the ingredients. The farmers in this neighbourhood prepare their ſeed wheat in one or other of the following methods.

Steeping in brine:

Formerly the wheat was immerſed about twelve hours in a ſtrong ſolution of common ſalt in water, and afterwards dried by mixing it with a ſufficient quantity of lime newly ſlaked.

wetting,

Of late years, in place of immerſing it, they pour a quantity of the ſaline ſolution over it on the floor, and after mixing the whole well together, dry it with lime as before.

or treatment with lime water only.

Another method which is now pretty generally adopted, is that in which no ſalt is uſed.

A quantity of boiling water is poured upon quick lime, and kept conſtantly ſtirred till the lime is reduced to powder, when it is immediately mixed with the grain. No great accuracy is uſed in aſcertaining the proportions; five or ſix pounds of lime, and three gallons of boiling water are about ſufficient to prepare five buſhels of wheat. In reaſoning *a priori*, one would be apt to ſuppoſe, that the vegetative powers of the grain would be materially injured by this boiling compoſition, but experience proves the contrary.

Experiment. Equal meaſures of ſound and ſmutty wheat were mixed. Half of this was ſteeped in brine for twelve hours, and half left unprepared.

Amidſt this diverſity of opinion on the cauſe of ſmut, I wiſhed to aſcertain the truth, if poſſible, by experiment. Accordingly, ſo early as December 1798, I mixed intimately together equal meaſures of ſound wheat and grains conſiſting entirely of ſmut. The heap was then divided into two equal parts; one of them was put into a ſaturated ſolution of ſalt in water for twelve hours, and then mixed with quick lime. The other part was ſubjected to no preparation whatever. Five or ſix days afterwards they were both planted in drills on a ſouth border, about nine or ten yards apart.

Both parcels were planted.

Both parcels came up about the ſame time, and while in blade, no difference could be perceived. While the ear was yet inveloped by the blade, I cautiouſly opened ſeveral of both crops, and in ſome of that which had undergone no preparation, a conſiderable difference was obſervable. Some of the embrio grains were opened, and in place of a milky juice, they contained only a ſmall quantity of a whitiſh ſubſtance, in which, by the help of a common magnifier, I could readily diſtinguiſh from one, to three or four black ſpecks in each. When rubbed between the fingers, a faint ſmell of ſmut was emitted. The ears which were examined, and had this appearance were marked, and afterwards proved to be ſmut. When the crops came out in ear, it was eaſy to diſtinguiſh the ſmut from the wheat. At the time of blowing no bloſſom whatever appeared on the ſmut ears, and the weather proving tempeſtuous at that time, the bloſſom was frequently waſhed off the wheat ears by heavy ſhowers, and as often renewed.

The unprepared ſeed gave an unhealthy product.

Smut eaſily diſtinguiſhed.

Both pieces were cut at the uſual time, and upon a careful examination, *that which had been ſubjected to no preparation conſiſted of nearly two-thirds of ſmut ears, the remainder being tolerably good wheat. In that which had been prepared, not a ſingle ſmut-ball could be found.*

The prepared ſeed produced ſound grain, and the unprepared ſeed ſmutty grain.

An accidental occurrence may be mentioned in corroboration of this experiment. Happening to paſs through a ſmall field of wheat juſt before the commencement of the harveſt, I was ſtruck with the unuſual quantity of ſmut in one part of it. On cloſe examination, I found that this extraordinary crop of ſmut ended abruptly in a line along one of the furrows. The other parts of the field had much the ſame appearance of others in the neighbourhood; a few ſmut ears ſcattered through it. Upon enquiry I found, that the ſeed with which this field had been ſown, running ſhort, the piece ſo abundant in ſmut had been ſown in ſeed which contained a conſiderable quantity of ſmut, and had undergone no preparation, only ſprinkling it with a little ſlaked lime immediately before ſowing.

An accidental occurrence of much ſmut confined to a part of a field, which had been ſown with unprepared ſeed.

The diſeaſe of ſmut is entirely confined to the grain. The ſtraw and every other part of the plant is ſound, and arrives at the natural ſize. Smut ears are *ſtaring*, and of a dirty whitiſh colour, inclining to blue, at the time when healthy ears are of a bright yellow. Their odour is fœtid, and not inaptly compared to that of ſtale lobſters. Part of an ear is not unfrequently found to contain ſmut, while the other parts are filled with ſound wheat.

Smut affects the grain only.

Deſcription of the ſmut.

Diſeaſed grains have more the globular form than thoſe of ſound wheat, which is perhaps the reaſon why they are called ſmut-balls. The ſkin is ſhrivelled and of a dirty browniſh hue, without any perforations which can be diſcovered by a high magnifying power. The whole of their contents, in a recent ſtate, are a blackiſh ſoft ſubſtance with a few ſhining ſpecks, which diſappear when dried.

The duſt of ſmut conſiſts of globules,

When kept ſome time in a dry place, this ſoft ſubſtance is in the form of a fine duſt or powder, of a dark brown colour when ſpread out on glaſs or talc. The microſcope ſhews each of theſe minute particles to be well formed globules, ſomewhat larger than the ſanguineous.

heavier than water:

They are ſpecifically heavier than water with which they readily mix but ſoon ſubſides, ſuffering no change by being kept in that fluid. In the beginning of September laſt, I infuſed ſome of the powder in water in a watch-glaſs. A few hours after I diſcovered by the microſcope, in a drop of the fluid a few animalculæ. Upon examination next day every drop of the liquor contained innumerable animalculæ, generally very minute but ſome a ſize larger. After ſtanding expoſed ſome days, the water evaporated, and an hour or two after the addition of freſh water every part ſwarmed with animalculæ, moving nimbly in all directions. While viewing them in the microſcope they ſuddenly became motionleſs owing to the evaporation of the drop of liquid; on adding a drop of freſh water, they inſtantly revived and began the ſame lively motion. A quantity of ſalt ſufficient to ſaturate the water was then added to the mixture. Upon examination about twenty hours afterwards, I was much ſurprized to find the animalculæ as numerous and lively as before the addition of the ſalt.

in which they produce animalcules,

which are not killed by ſalt;

The watch glaſs with its contents, after ſtanding neglected, on a ſhelf expoſed to the effluvia of a variety of drugs, till the latter end of November, was again filled with water, and placed near a fire, placing at the ſame time by it a ſimilar glaſs, containing ſmut powder and freſh water. They were both frequently examined for ſome days, but without diſcovering any animalculæ. My attention being called off by other avocations they remained unnoticed about eight days. The glaſs which contained the infuſion with ſimple water was quite dry, and only a ſmall quantity of fluid remained in the other. A drop being examined in the microſcope by a ſingle lens of a high magnifying power, was found to ſwarm with animalculæ. Both glaſſes were now filled with freſh water, and placed under inverted jars. Being examined two days after, each of them ſwarmed with lively animalculæ. While viewing them, *a ſmall particle of lime water was added to the drop, which proved inſtantly fatal*, at leaſt all motion ceaſed inſtantaneouſly, and was not renewed.

Lime water killed the animalcules from ſmut.

Among other inferences which may be drawn from the preceding facts and obſervations, are firſt, that the cauſe of ſmut is in the ſeed, and that ſmut produces ſmut in the crop. At the ſame time it is readily admitted, that certain ſeaſons are more favourable to ſmut than others, which can only be conſidered as a ſecondary cauſe. 2. That lime uſed in the manner above mentioned, prevents ſmut, if not entirely, at leaſt ſo far as not to prove injurious.

Inference. Smut is cauſed by bad ſeed, and lime water prevents it.

Is ſmut occaſioned by animalculæ? Some of the foregoing facts ſeem ſtrongly to favour the idea *.

V.

On the Diſcovery of Palladium; with Obſervations on other Subſtances found with Platina. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S* †.

HAVING ſome time ſince purified a large quantity of platina by precipitation, I have had an opportunity of obſerving various circumſtances in the ſolution of this ſingular mineral, that have not been noticed by others, and which, I think, cannot fail to be intereſting to this Society.

The principal ſubject of the preſent memoir is palladium.

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* Mr. Nicholſon will readily perceive that the ſubject is not near exhauſted. If future inveſtigation ſhould preſent any thing worthy of communication on the ſubject, ſhould Mr. N. deem ſuch deſerving a place in the Philoſophical Journal, he has only to mention it in a marginal line.

Anſwer. The diſeaſes of corn form a ſubject of ſuch high importance, whether conſidered in an economical or ſcientific point of view, that I muſt conſider it a duty to pay the moſt marked attention to whatever may tend to elucidate it.—N.

† Philoſ. Tranſ. 1805, p. 316.

As I have already given an account * of one product obtained from that ore, which I conſidered as a new metallic ſubſtance, and denominated Rhodium, I ſhall on the preſent occaſion confine myſelf principally to thoſe proceſſes by which I originally detected, and ſubſequently obtained another metal, to which I gave the name of *Palladium*, from the planet that had been diſcovered nearly at the ſame time by Dr. Olbers.

In the courſe of my inquiries I have alſo examined the many impurities that are uſually mixed with the grains of platina, but I ſhall not think it neceſſary to deſcribe minutely ſubſtances which have already been fully examined by others.

§ I. *Ore of Iridium.*

Ore of iridium, reſembles that of platina, but is inſoluble in nitro-muriatic acid; grains, harder to the file, not malleable and peculiar in their fracture:

I muſt however notice one ore, that I find accompanies the ore of platina, but has paſſed unobſerved from its great reſemblance to the grains of platina, and on that account is ſcarcely to be diſtinguiſhed or ſeparated from them, excepting by ſolution of the platina; for the grains of which I ſpeak are wholly inſoluble in nitro-muriatic acid. When tried by the file, they are harder than the grains of platina; under the hammer they are not in the leaſt degree malleable; and in the fracture they appear to conſiſt of laminæ poſſeſſing a peculiar luſtre; ſo that although the greater number of them cannot, as I have before obſerved, be diſtinguiſhed from the grains of platina, the laminated ſtructure ſometimes occaſions an external form by which they may be detected. With a view to be abſolutely certain that there exiſt grains in a natural ſtate, which have not been detached by ſolution from the ſubſtance of the grains of platina, I have ſeparated from the mixed ore as many as enabled me to aſcertain their general compoſition.

much heavier than the grains of platina;

Their moſt remarkable quality is their great ſpecific gravity, which I have found to be as much as 19,5, while that of the crude grains of platina has not, in any experiment that I have made, exceeded 17,7. From this circumſtance it might naturally be conjectured that they contain a greater quantity of platina than the grains in general; by analyſis, however, they do not appear to me to contain the ſmalleſt quantity of that metal, but

of which metal they contain none.

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* See our Journal, IV. 107.

to be an ore conſiſting entirely of the metals that were found by Mr. Tennant in the black powder which is extricated by ſolution from the grains of platina, and which he has called Iridium and Oſmium. But, ſince the ſpecific gravity of theſe grains ſo much exceeds that of the powder, which by my experiments has appeared to be, at the utmoſt, 14,2. I have thought it might deſerve inquiry whether their chemical compoſition is in any reſpect different. For this purpoſe I have ſelected a portion of them, and have requeſted Mr. Tennant to undertake a comparative examination, from whoſe well known ſkill in chemical inquiries, as well as peculiar knowledge of the ſubject, we have every reaſon to expect a complete analyſis of this ore.

§ II. *Hyacinths.*

Very ſmall hyacinths found among the platina grains;

Among thoſe bodies which may be ſeparated from the ore of platina, in conſequence of their leſs ſpecific gravity, by a current of water or of air, there may be diſcerned a ſmall proportion of red cryſtals ſo minute, that 100 of the largeſt I could collect weighed ſcarcely $\frac{3}{15}$ of a grain. The quantity which I poſſeſs is conſequently too ſmall for chemical analyſis; but their phyſical properties are ſuch as correſpond in every reſpect with thoſe of the hyacinth. I was firſt led to compare them with that ſtone by their ſpecific gravity, which I conjectured to be conſiderable from their accompanying other ſubſtances, that appear to have been collected together ſolely by reaſon of their ſuperior weight.

Like the hyacinth, theſe cryſtals loſe their colour immediately and entirely when heated; they alſo agree with it in their hardneſs, which is barely ſufficient to ſcratch quartz, but is decidedly inferior to that of the topaz.

The principal varieties of their form may be very well underſtood by deſcription.

Varieties of their forms;

1ſt. In its moſt ſimple ſtate the cryſtal may be conſidered as a rectangular priſm terminated by a quadrilateral obtuſe pyramid, the ſides of which ſometimes ariſe direct from the ſides of the priſm; but,

2dly. The poſition of the pyramid is generally ſuch that its ſides ariſe from the angles of the priſm, In this caſe the ſides of the priſm are hexagons.

3dly. It is more uſual for the priſm to have eight ſides by truncation of each of its angles, and at each extremity eight additional ſurfaces occupying the place of the eight linear angles between the priſm and terminating pyramid of the ſecond variety. The complete cryſtal has then thirty-two ſides.

4thly. The eight ſurfaces laſt mentioned, as interpoſed between the priſm and pyramid, are ſometimes elongated into a complete acute pyramid having eight ſides ariſing from the angles of an octahedral priſm.

which prove the nature of the ſtone.

The third form above deſcribed, correſponds ſo entirely with that given by the Abbé Haüy[*] as one of the forms of the hyacinth or jargon, that I have little reaſon to regret my inability to obtain chemical evidence of the compoſition of theſe cryſtals.

Thoſe, and other impurities, I uſually ſeparated, as far as was practicable, by mechanical means, previouſly to forming the ſolution of platina, which has been the principal object of my attention.

§ III. *Precipitation of Platina.*

Account of the treatment of platina. After the firſt precipitation of it by ſal ammoniac: another portion was thrown down by iron.

When a conſiderable quantity of the ore has been diſſolved, and I had obtained, in the form of a yellow triple ſalt, as much of the platina as could be precipitated by ſal ammoniac, clean bars of iron were next immerſed in the ſolution for the purpoſe of precipitating the remainder of the platina.

This is called the firſt metallic precipitate, and was again diſſolved as the firſt ore had been, and again precipitated by ſal ammoniac.

For diſtinction it will be convenient to call this, which in fact conſiſts of various metals, the firſt metallic precipitate. The treatment of this precipitate differed in no reſpect from that of the original ore. It was diſſolved as before, and a portion of platina precipitated by ſal ammoniac; but it was obſervable that the precipitate now obtained was not of ſo pale a yellow as the preceding. Nevertheleſs the impurity was in ſo ſmall quantity, that the platina reduced from it by heat did not differ diſcernibly from that obtained from the pureſt yellow precipitate.

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[*] *Traité de Mineralogie*, Pl. XLI. fig. 17. *Journ. des Mines*, No. 26, fig. 9.

At this time I found it advantageous to neutralize the ſolution with ſoda, and to employ a ſolution of green ſulphate of iron for the precipitation of the gold, of which, I believe, a portion may always be obtained from the mixed ore; but I have obſerved in experiments upon any quantities of mere grains of crude platina carefully ſelected, that the ſmalleſt portion of gold could not be detected as a conſtituent part of the ore itſelf.

The ſolution was neutralized with ſoda; and gold, if preſent, precipitated by ſolution of green ſulphate of iron.

Bars of iron were ſubſequently employed as before for recovering the platina that remained diſſolved, together with thoſe ſubſtances which I have ſince found to accompany it.

A ſecond metallic precipitate was thrown down by iron.

The precipitate thus obtained, which I diſtinguiſh by the name of the ſecond metallic precipitate, was to appearance of a blacker colour than the former, and was a finer powder.

As I was not at firſt prepared to expect any new bodies, I proceeded to treat the ſecond precipitate, as the former, by ſolution and precipitation. But I ſoon obſerved appearances which I could not explain by ſuppoſition of the preſence of any known bodies, and was led to form conjectures of future diſcoveries, which ſubſequent inquiry has fully confirmed.

When I attempted to diſſolve this ſecond metallic precipitate in nitro-muriatic acid, I was ſurpriſed to find that a part of it reſiſted the action of that ſolvent, notwithſtanding any variations in the relative proportions or ſtrength of the acids employed to form the compound, and although the whole of this powder had certainly been twice completely diſſolved.

This was not all ſoluble in nitro-muriatic acid.

The ſolution formed in this caſe was of a peculiarly dark colour, and when I endeavoured to precipitate the platina from it by ſal ammoniac, the precipitate obtained was ſmall in quantity, and, inſtead of being yellow, was of deep red colour, ariſing from an impurity which I did not at that time underſtand, but which we ſince know, from the experiments of Mr. Deſcotils, is occaſioned by the metal now called iridium.

This ſolution was very dark and its precipitation by ſal ammoniac was deep red, occaſioned by iridium.

The ſolution, inſtead of being rendered pale by the precipitation of the platina, retained its dark colour in conſequence of the other metals that remained in ſolution; but, as I had not then learned the means of ſeparating them from each other, and as the quantity of fluid which accumulated occaſioned me ſome inconvenience, I decompoſed it by iron, as

Precipitation of a third metallic precipitate by iron.

in the former inſtances, and formed a third metallic precipitate, which could more commodiouſly be reſerved for ſubſequent examination.

Much of this being rhodium, was inſoluble.

In this laſt ſtep I committed an error which afterwards occaſioned me conſiderable difficulty, for I found that a great part of this precipitate conſiſting of rhodium was unexpectedly rendered inſoluble by this treatment, and reſembled the reſiduum of the ſecond metallic precipitate abovementioned.

As I have already communicated to this ſociety, in my Paper upon rhodium, the proceſs by which I ſubſequently avoided this difficulty, I ſhall at preſent return to a previous ſtage of my progreſs, and relate the means by which I firſt obtained palladium in my attempts to analyze the ſecond metallic precipitate.

§ IV. *Separation of Palladium.*

Separation of palladium. The ſecond metallic precipitate contained lead, copper, and another metal precipitable by copper.

There was no difficulty in aſcertaining the preſence of lead as one of the ingredients of this precipitate, by means of muriatic acid, which diſſolved lead and iron and a ſmall quantity of copper. It was equally eaſy to obtain a larger portion of copper by dilute nitrous acid, with which it formed as uſual a blue ſolution. But when I endeavoured to extract the whole of the copper by a ſtronger acid, it was evident, from the dark brown colour of the ſolution, that ſome other metallic ingredient had alſo been diſſolved. I at firſt aſcribed this colour to iron; but, when I conſidered that this ſubſtance had been more ſlowly acted upon than copper, I relinquiſhed that hypotheſis, and endeavouring to precipitate a portion of it by a clean plate of copper, I obtained a black powder adhering to a ſurface of platina on which I had placed the ſolution. As this precipitate was ſoluble in nitric acid, it evidently conſiſted neither of gold nor platina; as the ſolution in that acid was of a red colour, the metal could not be either ſilver or mercury; and as the precipitation of it by copper excluded the ſuppoſition of all other known metals, I had reaſon to ſuſpect the preſence of ſome new body, but was not fully ſatisfied of its exiſtence until I attempted the precipitation of it by mercury.

It was ſeparated by agitating mercury with the ſolution with

For this purpoſe I agitated a ſmall quantity of mercury in the nitrous ſolution previouſly warmed, and obſerved the mercury to acquire the conſiſtence of an amalgam. After this amalgam had been expoſed to a red heat, there remained a white metal, which could not be fuſed before the blowpipe. It gave a red ſolution as before in nitrous acid; it was not precipitated by ſal ammoniac, or by nitre; but by pruſſiate of potaſh it gave a yellow or orange precipitate; and in the order of its affinities it was precipitated by mercury but not by ſilver.

which it formed an amalgam; and the mercury was driven of by heat. It was palladium.

Theſe are the properties by which I originally diſtinguiſhed palladium; and by the aſſiſtance of theſe properties I obtained a ſufficient quantity for inveſtigating its nature more fully.

There were, however, various reaſons which induced me to relinquiſh the original proceſs of ſolution in nitrous acid and precipitation by mercury; for although I found the metal thus obtained to be nearly pure, the neceſſity of agitating the ſolution with the mercury was very tedious, and the waſte was alſo conſiderable; for in the firſt place it ſeemed that nitrous acid would not extract all the palladium from any quantity of the ſecond metallic precipitate, neither would mercury reduce the whole of what was ſo diſſolved. I therefore ſubſtituted a proceſs dependent on another of its properties, I had obſerved that this metal differed from platina in not being precipitated from nitro-muriatic acid by nitre or by other ſalts containing potaſh; for although a triple ſalt is thus formed, this ſalt is extremely ſoluble, while that of platina on the contrary requires a large quantity of water for its ſolution. On that account a compound *menſtruum* conſiſting of nitrate of potaſh diſſolved in muriatic acid is unfit for the ſolution of platina, but diſſolves palladium nearly as well as common nitro-muriatic acid in which there is no potaſh preſent *.

The proceſs with mercury was abandoned;

In five ounces of muriatic acid diluted with an equal quantity of water, I diſſolved one ounce of nitre, and formed a ſolvent for palladium that poſſeſſes little power of acting on platina, ſo that by digeſting any quantity of the ſecond metallic precipitate till there appeared to be no farther action, I procured a ſolution from which by due evaporation were formed cryſtals of a triple ſalt, conſiſting of palladium combined with muriatic acid and potaſh. Theſe are the cryſtals which I have on a

and a ſolvent conſiſting of muriatic acid, with nitre was uſed to the ſecond precipitation from which it takes palladium but not platina.

The ſolution gave cryſtals of triple ſalt of palladium potaſh and muriatic acid.

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* I have found that gold may alſo be diſſolved with equal facility by the ſame ſolvent, and nearly in the ſame proportion. Ten grains of nitre added to a proper quantity of muriatic acid are ſufficient for ſixteen grains of either gold or palladium.

former occaſion[*] mentioned as exhibiting a very ſingular contraſt of colours, being bright green when ſeen tranverſely, but red in the direction of their axis; the general aſpect, however, of large cryſtals is dark brown.

From the ſalt thus formed and purified by a ſecond cryſtallization, the metal may be precipitated nearly pure by iron or by zinc, or it may be rendered ſo by ſubſequent digeſtion in muriatic acid.

§ V. *Reaſons for thinking Palladium a ſimple Metal.*

That palladium is a ſimple metal appears from,

its forming a diſtinctly cryſtallized ſalt with baſes and an acid,

its combinations with metals and ſeparation without change.

From the conſideration of this ſalt alone I thought it highly probable that the ſubſtance combined in it with muriate of potaſh was a ſimple metal, for I know of *no inſtance in chemiſtry of a diſtinctly cryſtallized ſalt containing more than two baſes combined with one acid.* I nevertheleſs endeavoured by a ſuitable courſe of experiments to obviate all probable objections. After examining by what acids it might be diſſolved and by what reagents it might be precipitated, I combined it with various metals, with platina, with gold, with ſilver, with copper, and with lead; and when I had recovered it from its alloys ſo formed, I aſcertained that, after every mode of trial it ſtill retained its characteriſtic properties, being ſoluble in nitrous acid, and precipitable from thence by mercury, by green ſulphate of iron, by muriate of tin, by pruſſiate of potaſh, by each of the pure alkalis, and hydroſulphurets.

and its precipitation is reducible by mere heat.

The precipitate obtained in each caſe was alſo found to be reducible by mere heat to a white metal, that, except in very ſmall quantities, could not be fuſed alone by the blowpipe, but could very readily be fuſed with ſulphur, with arſenic, or with phoſphorus, and in all other reſpects reſembled the original metal.

Qu. Whether it might conſiſt of metal and a fixed acid?

The only hypotheſis, on which I thought it poſſible that I could be deceived, aroſe from the recollection of the error, which ſubſiſted for a few years, reſpecting the compound formerly called ſiderite. It was poſſible that ſome metallic or other fixed acid might unite too intimately with either a known or an unknown metal to be ſeparated by the more common ſimple affinities. I conſequently made ſuch attempts as appeared beſt calculated to diſunite a compound ſo conſtituted.

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[*] Phil. Trans. 1804, p. 428.

Having boiled the oxide with pure alkalis, and found it to be unaltered, I thought the affinities of lime or lead might be more likely to detect the preſence of the phoſphoric or of any known metallic acid; and accordingly I made various attempts by muriate and nitrate of lime, as well as by nitrate of lead, to effect a decompoſition of the ſuppoſed compound. In the experiment on which I placed the greateſt reliance, I poured liquid muriate of lime into a ſolution of palladium in nitro-muriatic acid, and evaporated the mixture to dryneſs, intending thereby to expel any exceſs of acid that might have been left in the ſolution, and to render either phoſphate of lime, or any compound of lime with a metallic acid, inſoluble in water. The reſiduum however was very readily diſſolved by water, and conſiſted merely of muriate of lime and muriate of palladium, without any appearance of decompoſition.

The oxide is not affected by boiling with alkalis; nor by pouring muriate of lime into its ſolution.

When I found all my endeavours directed to that end wholly unſucceſsful, I no longer entertained any doubt of this ſubſtance being a new ſimple metal, and accordingly publiſhed a conciſe delineation of its character; but by not directing the attention of chemiſts to the ſubſtance from which it had been extracted, I reſerved to myſelf an opportunity of examining more at leiſure many anomalous phenomena, that had occurred to me in the analyſis of platina, which I was at a loſs to explain, until I had learned to diſtinguiſh thoſe peculiarities, that I afterwards found to ariſe from the preſence of rhodium.

Hence the diſcoverer was warranted in publiſhing it as a new metal.

§ VI. *Additional Properties of Palladium.*

In my former Paper on that ſubject I alſo added ſome obſervations upon the properties and origin of palladium, deſcribing only ſuch a mode of obtaining it from platina as ſhould avoid the introduction of any unneceſſary ingredient which might poſſibly be miſinterpreted, and omitted one of the moſt diſtinguiſhing properties of palladium, by means of which it may be obtained with the utmoſt facility by any one who poſſeſſes a ſufficient quantity of the ore of platina.

Method of eaſily ſeparating palladium.

To a ſolution of crude platina, whether rendered neutral by evaporation of redundant acid, or ſaturated by addition of potaſh, of ſoda, or ammonia, by lime or magneſia, by mercury, by copper, or by iron, and alſo whether the platina has or has not been precipitated from the ſolution by ſal ammoniac, it is merely neceſſary to add a ſolution of pruſſiate of mercury, for the

Pruſſiate of mercury added to the ſolution of crude platina, throws down the pure pruſſiate of palladium;

the precipitation of the palladium. Generally for a few ſeconds, and ſometimes for a few minutes, there will be no appearance of any precipitate; but in a ſhort time the whole ſolution becomes ſlightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowiſh-white colour. This precipitate conſiſts wholly of pruſſiate of palladium, and when heated will be found to yield that metal in a pure ſtate, amounting to about four or five tenths *per cent.* upon the quantity of ore diſſolved.

Heat diſengages the pure metal which is not more than one two hund. part of the original ore.

The pruſſiate of mercury is peculiarly adapted to the precipitation of palladium, excluſive of all other metals, on account of the great affinity of mercury for the pruſſic acid, which in this caſe prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium that I have ſtated by uſing more of the pruſſiate of mercury, or to procure any precipitate by the ſame means from a ſolution of pure platina.

More mercury does not augment the product.

The decompoſition of muriate of palladium by pruſſiate of mercury is not effected ſolely by the ſuperior affinity of mercury for the muriatic acid, but is aſſiſted alſo by the greater affinity of pruſſic acid for palladium; for I have found that pruſſiate of palladium may be formed by boiling a precipitated oxide of palladium in a ſolution of pruſſiate of mercury.

The decompoſition is by double affinity.

The pruſſiate of mercury is conſequently a teſt by which the preſence of palladium may be detected in any of its ſolutions; but it may be worth obſerving, that the precipitate obtained has not in all caſes the ſame properties. In general, this compound is affected by heat ſimilarly to other pruſſiates, but when the palladium has been diſſolved in nitrous acid and precipitated from a neutral ſolution by pruſſiate of mercury, the precipitate thus formed has the property of detonating when heated. The noiſe is ſimilar to that occaſioned by firing an equal quantity of gunpowder, and accordingly the exploſion is attended with no marks of violence unleſs occaſioned by cloſe confinement. The heat requiſite for this purpoſe is barely ſufficient to melt biſmuth, conſequently is about 500° of Fahrenheit. The light produced is proportionally feeble, and can only be ſeen in the abſence of all other light.

Pruſſiate of mercury is the teſt of palladium.

The precipitation from a nitrous ſolution detonates by low heat.

Palladium is very ſlowly acted on by nitric acid and extricates no gas.

In endeavouring to diſſolve a piece of palladium in ſtrong colourleſs nitric acid for the purpoſe of forming the detonating pruſſiate, I found that, although the acid ſhortly acquired a red colour ſurrounding the metal, the action of the acid was extremely ſlow, and I was ſurpriſed to obſerve a fact that appears to me wholly ſingular; the metal was taken up without any extrication of nitrous gas; and this ſeemed to be the cauſe of the ſlow ſolution of this metal, as there was not that circulation of this fluid, which takes place in the ſolution of other metals until the acid is nearly ſaturated.

Nitrous acid acts more ſtrongly.

As the want of production of gas appeared to retard the ſolution of palladium, I tried the effect of impregnating a quantity of the ſame acid previouſly with nitrous gas, and obſerved its action to be very conſiderably augmented, although the experiment was neceſſarily tried in the cold, becauſe the gas would have been expelled by the application of heat.

Beſide thoſe properties which are peculiar to palladium there are others, not leſs remarkable, which it poſſeſſes in common with platina. I have on a former occaſion mentioned that theſe metals reſemble each other in deſtroying the colour of a large quantity of gold. Their reſemblance, however, in other properties is not leſs remarkable, more eſpecially in the little power they poſſeſs of conducting heat, and in the ſmall degree of expanſion to which they are liable when heated.

Conducting powers of palladium and platina as to heat, tried by the melting of wax upon them. The meaſure is not half thoſe of ſilver and copper.

For the purpoſe of making a compariſon of the conducting power of different metals, I endeavoured to employ them in ſuch a manner, that the ſame weight of each metal might expoſe the ſame extent of ſurface. With that view I ſelected pieces of ſilver, of copper, of palladium, and platina, which had been laminated ſo thin as to weigh each 10 grains to the ſquare inch. Of theſe I cut ſlips $\frac{1}{10}$ of an inch in breadth, and four inches long; and having covered their ſurfaces with wax, I heated one extremity ſo as to be viſibly red, and, obſerving the diſtance to which the wax was melted, I found that upon the ſilver it had melted as far as 3¼ inches: upon the copper 2½ inches: but upon the palladium and upon the platina only one inch each: a difference ſufficient to eſtabliſh the peculiarity of theſe metals, although the conducting power cannot be ſaid to be ſimply in proportion to thoſe diſtances.

Rate of expanſion by heat, tried by rivetting bars together.

In order to form ſome eſtimate of the comparative rate of expanſion of theſe metals, I rivetted together two thin plates of

platina

While ſteel expands through 12, platina will expand 9, and palladium 10.

platina and of palladium; and obſerving that the compound plate, when heated, became concave on the ſide of the platina, I aſcertained that the expanſion of palladium is in ſome degree the greater of the two. By a ſimilar mode of compariſon I found that palladium expands conſiderably leſs than ſteel by heat; ſo that if the expanſion of platina between the temperatures of freezing and boiling water be eſtimated at 9 parts in 10,000, while that of ſteel is known to be about 12, the expanſion of palladium will probably not be much more or leſs than 10, or one part in 1000 by the ſame difference of temperature.

It muſt, however, be acknowledged, that the method I have purſued is by no means ſufficient for determining the preciſe quantity of expanſion of any ſubſtance; but I have not been induced to beſtow much time on ſuch an inquiry, ſince the extreme ſcarcity of palladium precludes all chance of any practical utility to be derived from a more accurate inveſtigation.

VI.

Report made to the Athénée des Arts of Paris, by MM. RONDELET, BEAUVALLET, *and* DUCHESNE; *on the founding the Statue of* JOAN OF ARC *in Bronze, by a Way never before uſed for large Works, by MM.* ROUSSEAU *and* GENON, *under the Direction of M.* GOIS, *Statuary.**

Caſting in ſand uſed hitherto only for ſmall figures.

THE method of caſting in ſand hitherto has only been uſed for figures from 65 to 70 centimetres (about 2¼ feet) in heighth; while the ſtatue, which was to be formed, being of much larger dimenſions, ſhould of courſe be managed according to the method called the *great foundery*, on account of its being uſed for coloſſal ſtatues.

The great foundery known to the ancients, but was loſt.

At its revival large ſtatues were caſt in ſeparate parts.

This method of caſting was known to the ancients, who were even ſuperior to us in it; but this art was loſt with many others, and in the time of the Medicis large ſtatues were not formed at a ſingle caſting. The figures of Henry the Fourth and of Lewis the Thirteenth, which are ſeen at Paris, were placed on horſes made previouſly, one for the ſtatue of Ferdinand, Grand Duke of Tuſcany, and the other for that of Henry the Second, King of France.

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* Magazin Encyclopédique, T. I. p. 350.

The ſtatue of Lewis the Fourteenth, in the Place de Vendome, is the firſt that was formed at a ſingle caſting ſince the revival of the art. It was ſuitable to ſo great a prince to permit his image to be made ſolely by a grand method; but Girarden and Keller, to whom the work was entruſted, then made their firſt attempts, which occaſioned many faults, ſuch as the caſting it too thick, which in uſeleſsly employing more metal, increaſed the difficulty of ſupporting the coloſſal figure; and ſuch alſo as uſing unneceſſary labour; but notwithſtanding all their precautions, the caſting did not ſucceed perfectly, and conſiderable repairs were obliged to be made in it.

That of Lewis XIV. the firſt caſt after this in a ſingle piece.

About the ſame time were erected the equeſtrian ſtatues of this prince, at Boufflers and at Lyons, by the ſame Girarden, at Rennes by Coizevox, at Montpelier by Mazeline and Utrels, and at Dijon by Le Hongre.

Various other ſtatues made in the ſame way.

After this, Le Moine had to found a ſtatue of Lewis the Fifteenth at Bourdeaux, which met with great accidents; but he had more ſucceſs with one at Rennes, which was a pedeſtrian ſtatue: Guibal alſo made one for Nancy. But this art did not attain to a great perfection till Boucharden was employed to conſtruct an equeſtrian ſtatue of Lewis the Fifteenth at Paris: The great care of M. Goor prevented any accident from happening to it, or to that alſo which was caſt at Reims by the ſame artiſt, from the model of Pigale. This founder had not the ſame ſucceſs when he formed the ſtatue of Frederic the Fifth at Copenhagen, from the model of Saly, which required great repairs. Finally, great improvements had been made in the art when the ſtatue of Peter the Great was founded at Peterſburg by Falconet, and nevertheleſs he was obliged to refound a ſecond time the upper half of the ſtatue.

The art not very perfect before the ſtatue of Lewis XV. was caſt.

Great improvements made in the art when the ſtatue of Peter the Great was caſt.

The great diſadvantage of the method hitherto uſed, is its enormous expence and the great time it requires. It is true, that for works which are intended for duration economy is not the chief object; but if they can be performed equally well by M. Gois's method, at one-half the expence and in a fourth

Diſadvantages of the great foundery.

M. Gois's method ſuperior to it in various reſpects.

of

Reaſons for believing that it would anſwer for the largeſt works.

of the time, it certainly ought to be preferred. There is good reaſon alſo to decide, that this method will do equally well for the largeſt works; for, according to calculation, the largeſt ſtatue of this kind in France exceeds that of Joan of Arc by a much ſmaller proportion than the latter exceeds that of the largeſt ſtatues ever before caſt in the ſame manner, which never weighed more than from 8 to 10 kilogrammes (from about 17 to 22 lb.) The ſtatue of Joan of Arc weighed 600 kilogrammes, which is 60 times more; but that of Lewis the Fifteenth, which weighed 17,000 kilogrammes, was only twenty-eight times heavier than that of Joan of Arc.

But in order to judge better of the advantages of M. Gois's method, it ſhall be deſcribed at large, and an account given alſo of the method of molding by wax, or of the *grand foundery*, in order to compare them together.

M. Gois's ſtatue of Joan of Arc exhibited and admired.

M. Gois having made a ſtatue of Joan of Arc for a prize, exhibited it in public in the year 10, (1802.) The prefect of the department of the Loire ſaw it, and propoſed to the city of Orleans to re-erect that monument to the glory of this heroine, which had been deſtroyed in the anarchy of the revolution. It was accordingly ordered to be done. M. Gois being informed of this, went to Orleans and offered to make a caſt in bronze from his ſtatue, without preciſely knowing whether it was that which the city required.

He is employed to make a caſt in bronze from it for the city of Orleans.

An agreement was then made with M. Gois to complete the ſtatue at a fixed price, in the courſe of about one year from the 5th Germinal, *An XI.* or before May 4, 1804.

Is induced to have the caſt made in ſand to ſave time and expence.

M. Gois began to be alarmed at the enormous expence of the uſual method of caſting ſuch ſtatues, and at the great time it required, which he feared would prevent his performing the agreement. He knew that M. Rouſſeau had made a caſt from the groupe of Graces by Germain Pilon, with great ſucceſs, by a different method; and though theſe figures were but 1,38 metres high, and his ſtatue was more than two metres, (6½ feet) he went notwithſtanding to conſult this founder, who engaged for the ſucceſs of the method, and promiſed to employ in the buſineſs the ſame workman who had caſt the above groupe without having met with any accident: This laſt conſideration determined M. Gois to entruſt the work to the *founders in ſand*.

M. Rouſſeau undertakes the work for him, who had before made a fine caſt of the Graces; and employs the ſame workman who caſt them.

The firſt of Fructidor, *An XI.* the buſineſs was began; but as they commenced with the bas-reliefs, it was not till three months after that they undertook the work of the ſtatue.

The proceſs deſcribed for caſting Joan of Arc in ſand.

They made uſe of the common ſand of the founders, which is argillaceous, and always kept a little moiſt. After having well raked it, ſeparated all the ſtones, and broken all the lumps that could be met with, they filled with it a caſe of 2,20 metres long, and one metre broad at the inſide, and 16 centimetres high; the thickneſs of the wood of the caſe was eight centimetres. The ſand was ſtrongly beaten with a rammer 10 centimetres broad and 60 long, and by this operation acquired ſufficient conſiſtence to be raiſed along with the caſe without any danger of running out.

After this the ſtatue was placed upon the firſt caſe, which is called the *falſe mold,* becauſe it was to be afterwards replaced by another: the ſand was ſtirred up a little, to permit the moſt prominent parts to enter it; another caſe of the ſame ſize was then put over the firſt, and attached to it by four points of iron.

Method of forming the mold.

The true *concave mold* was then began, by modelling each part of the figure with the ſame kind of ſand. A workman of much addreſs and intelligence is requiſite for the diviſion and diſtribution of the different pieces which form the mold: he ſhould explain the motives which induced him to prefer one diſtribution to another: each piece ſhould have different ſections: care ſhould be taken to mold the parts which have a large and uniform ſurface in a ſingle piece, while the pieces muſt be multiplied for thoſe portions of the ſtatue which have many ſinuoſities and deep indentations.

This part of the operation requires the moſt care; for if it be performed with negligence, the extraction of the model would be attended with great difficulties; and if the workman employed is aukward, numerous faults will need reparation after the caſting, and probably great accidents may happen. It is but juſtice to ſay, that Genon, the workman on this ſtatue, ſhewed in his performance equal dexterity and knowledge.

To prevent the pieces of the mold from adhering to each other, care is taken to powder the parts of each which is finiſhed, with charcoal duſt incloſed in a bag, before a new piece is began. The workman having finiſhed the moldings of the contours of the figure, filled up the empty ſpaces between them and the caſe with ſand, which he firſt preſſed and forced together with his hands, afterwards beat it with the bat, and finally with a mallet; this compreſſion gives it ſuch a ſolidity that it appears like ſtone, or at leaſt like baked earth.

The ſame care was taken with each caſe as they were ſucceſſively added, to the number of ſeven; which compleating the top of the ſtatue, the whole was then reverſed in order to replace the loweſt caſe, which, as mentioned, was only a falſe mold; and then each part at the lower extremity was alſo modelled in ſame manner as the preceding.

The pieces of the mold cemented together with paſte, and ſecured by wires paſſing into the caſes.

The hollow mold being finiſhed, the caſes were taken aſunder, and each piece removed ſeparately to take out the ſtatue; then they were all placed in their proper order in the exterior mold, which may be compared to the cover uſed by thoſe who make plaſter-of-Paris caſts. Each piece would be well retained in its place by its irregular form; but it was ſtill farther faſtened by a little thin paſte made of flour, which was applied by a bruſh both to the pieces themſelves and the parts that adhered to the caſes. It was thought neceſſary to take a precaution more than what was uſual, through the apprehenſion that the paſte would not hold together thoſe large pieces as well as it did the ſmall pieces in leſſer works; they were therefore traverſed by long wires of iron, which entered into the cover or exterior mold.

Method of forming the core.

This mold being thus entirely compleated, had only to be dried till the time of the caſting. A new mold was neceſſary to be made to caſt the core: the ſame pains were not taken with this as with the firſt, as it would be uſeleſs to do ſo. When this ſecond mold was finiſhed, a coat of modelling-clay was applied to its inſide, of the ſame thickneſs which was intended to be given to the bronze; and without waiting for its drying, it was cloſed and the core caſt in it, which was compoſed, as is uſual, of equal parts of plaſter of Paris and of brick duſt.

Eight iron rods laid ſo as to project a few inches from the core, in order to place it in the mold.

Eight rods of iron having been placed at the inſide of the mold, afterwards projected from the core about 10 or 12 centimetres, which ſerved to place it with preciſion in the hollow mold.

But in the mean time the caſes had been placed one over the other, and the iron pins which connected them fitted to their places, taking care to divide them into two portions, which anſwered to the two caſes uſually employed by the founders; and which, inſtead of the uſual thickneſs of five centimetres at moſt, were, the one 48 and the other 64 centimetres thick.

The pieces of the mold dried.

In this ſtate they were dried, by placing them round a braſier of kindled charcoal, the fire of which had the more power from the mold being divided into two portions, and empty.

The core alſo dried.

The core was likewiſe dried by placing it over a braſier of charcoal; the ſame was alſo placed round it; and in eight hours the moiſture was entirely evaporated. It was left to cool, and it was placed in one half of the mold; the ſecond half of the mold was afterwards fitted on, and the whole compreſſed together by iron preſſes in the uſual manner.

The mold and core put together ready for caſting, and ſecured by preſſes.

After this there only remained to conſtruct the baſon, *(l'echino)*, to fuſe the metal, and make the caſt. Theſe operations being the ſame for both methods of founding, ſhall be related, after firſt as briefly as poſſible deſcribing the method of caſting by the great foundery in which wax is uſed.

Deſcription of caſting by the great foundery.

The firſt operation for the great foundery is to dig a trench proportionate to the ſize of the figure to be caſt, and to ſurround it with a wall to prevent the earth from tumbling in.

A mold formed from the ſtatue in ſeparate pieces.

After the model is finiſhed it is oiled, and a mold formed from it with plaſter of Paris in the uſual way, and with the precautions before directed for molding in ſand: In each piece of this mold rods of iron are inſerted, by which they may be eaſily lifted when the mold is taken aſunder or put up; each of theſe pieces is numbered, that its proper place may be known.

A compoſition of wax laid on to the inſide of the mold, of the thickneſs intended for the bronze.

After this ſeveral layers are applied with a bruſh to the inſide of the pieces, of a compoſition made of 7-tenths of yellow wax, 1-tenth of turpentine, 1-tenth of white pitch, and 1-tenth of hog's-lard, which is melted ſlowly to prevent its forming bubbles.

When the different layers form a thickneſs of three or four millimetres, (0.15 inch) cakes of wax are placed inſide in thoſe parts where the bronze ſhould be of a greater thickneſs, and faſtenings of ſheet-braſs are inſerted, which may take hold of the core and prevent the wax from falling off.

The core caſt in this mold, and the modelling of the wax finiſhed by the ſtatuary after it is taken out.

The mold is then fitted together, and the core caſt with quickneſs, that it may form an entire maſs, and not lie in layers: As ſoon as it is ſolid the model is taken aſunder, and the ſtatuary repairs the wax, takes off all the ſutures of the molds, rectifies the errors which may have occurred, and gives to the work all the perfection of which it is ſuſceptible.

The true mold formed over the wax; its compoſition

After this the true mold is formed of materials capable of ſupporting the heat and the impulſe of the metal; to compoſe which three-ſixths of earth are mixed with one-ſixth of horſe-dung, and left to rot in a ditch for one winter: when this mixture is taken out, two-ſixths of broken crucibles, well pounded and paſſed through a ſieve, are added: the whole is tempered with urine and beat up on a ſtone: it is then what is called *potee*.

Formation of the true mold continued.

When it is to be uſed, a ſufficient quantity is taken and water enough added to it, to make it capable of being laid on with a bruſh; forty coats of it are then laid over the wax ſucceſſively, (care being taken that one coat is dry before another is laid on), which altogether form a thickneſs of five centimetres (2 inches.) The mold is then ſurrounded with flat bands of iron, which croſs each other like net-work; then, after rendering the *potee* thicker, by adding earth to it, and hair that has been well beaten, it is laid on over the former work with the fingers, until the mold has attained the thickneſs of twenty centimetres below and ſixteen above (6 and 8 inches); after which it is ſurrounded a ſecond time with bands of flat iron.

Preparations for, and method of melting out the wax.

After this a wall is built round it, the paſſages neceſſary for the fire conſtructed, and the intervening ſpaces are filled up with broken bricks: Then the fire is kindled in the paſſages moſt diſtant from the figure, and is gradually increaſed for nine days, and again diminiſhed for the ſame ſpace of time. On the ſecond day the wax begins to flow, and continues to do ſo for ten or twelve days; about half of it is loſt.

When the fire is extinguiſhed, the work is left ſome days to cool; then the broken bricks are removed, and before the mold is buried, a coat of plaſter, about half an inch thick, is put over it, which is called the *chemiſe*. Then they proceed to bury the mold, or incloſe it with earth, firſt taking care to ſtop all the ways through which the wax flowed, and to raiſe the

The mold buried, and the pipes and vents placed ready for caſting.

the pipes for the vents and for the entrance of the fuſed metal. The earth uſed for encloſing the mold ſhould be firſt ſkreened, and then laid on equally in the excavation. After each courſe is raiſed to a thickneſs of thirty centimetres, (1 foot) it is beaten down till it is reduced to ten.

After this there only remains to build the baſon for the reception of the metal, called the *echino*.

Enumeration of the various advantages of caſting in ſand, and diſadvantages of the great foundery.

In enumerating the operations neceſſary for the method of caſting in ſand firſt mentioned, the authors of the report ſtate them to amount to ten; while thoſe uſed for caſting in the large way laſt recited, in which wax is uſed, amount to no leſs than twenty-eight, each of which they particularize; but as theſe operations may readily be counted from the relation already given, this catalogue is not inſerted here. They alſo remark, that the laying on the wax on the pieces of the mold takes up much time, as does likewiſe the preparation of the *potee:* that in the firſt method the circling with bands of iron is entirely avoided, and the building of the paſſages for the fire, which are very expenſive: that likewiſe the molding and ſetting up of the vaſt number of pipes and vents is ſaved in it; and in the drying of the work the economiſing of fuel is greatly in favour of the firſt method, for in it theſe operations are performed in a ſhort time with a very ſmall fire, which in the other method require at leaſt three weeks and a powerful heat: that in the repairing of the wax the ſtatuary muſt work with his own hands: and that in taking out the ſtatue when caſt, there is vaſtly leſs trouble in the firſt method.

The authors here deſcribe the method of erecting the furnace for fuſing the metal for the ſtatue of Joan of Arc; but as it was conſtructed to burn wood, which fuel is not uſed in our founderies, and as the deſcription would be on other accounts of but little benefit to our artiſts, it is omitted.

It is only neceſſary to ſtate, that the place which contained the fuſed metal was at ſuch an elevation, that, when the ſtopper which retained it was driven in, it might flow freely into the *echino* through the paſſage prepared for it.

The mold of the ſtatue of Joan of Arc laid in the earth ready for caſting.

The mold for the ſtatue was partly buried in the earth, ſo as to allow a fall for the metal of eight centimetres (3 in.) from the hearth to the entrance of the pipes; and the authors obſerve, that the trouble of burying the mold might be avoided by laying it on its ſide, for which poſition they think it was ſufficiently well prepared.

Parts of the ſtatue caſt ſeparate from the reſt.

The ſtatue was all formed in one mold, except the ſkirts, (*pleinte*), one arm, and the plumes of the helmet, which were placed in a ſeparate caſe: This might have been diſpenſed with, but it was apprehended that, if they remained with the ſtatue, they would have much encreaſed the difficulty of the work, by adding to the elevation of the figure.

1000 kilograms of metal fuſed.

Every thing being prepared for the caſting, about 1000 kilograms (about 32 C.) of the metal was placed in the furnace, one half of which metal conſiſted of old cannon, a third of copper, and the reſt of braſs; and on the 8th of Germinal, *An XII.* (29th March, 1804), at eight o'clock in the afternoon, the metal, after five hours heating, being in compleat fuſion, and the *echino* and the ſtoppers which cloſed the two paſſages for the metal being previouſly heated, M. Rouſſeau forced in the plug that retained the metal in the furnace; it flowed immediately into the *echino*; the ſtoppers were removed from the paſſages for the metal from thence, and in leſs than two minutes a little of the metal appeared at the vents, and ſhewed that the caſt was completed.

The caſt made by M. Rouſſeau: It ſucceeded compleatly.

The ſtatue requires no repairs or additions.

On removing the ſand it was found that no accident had happened but a ſlight flaw on the ſtomach of the figure; that the head was quite perfect; and that there had been no partial caſting, or any part of the figure neceſſary to replace, which often happens in the other method.

The reporters recommend the *Athénée des Arts* to give medals to MM. Rouſſeau and Genon, and to make honourable mention of M. Gois.

The reporters conclude with high encomiums on the advantages of this method of caſting, and recommend that medals be given to MM. Rouſſeau and Genon by the Athénée des Arts, in teſtimony of their merit; and that as the rules of the Society prevented this recompence from being granted to any of its members, honourable mention ſhould be made of M. Gois.

VII.

Experiments made at the Galvanic Society of Paris, by M. Riffant, *Director of the Nitre and Gunpowder Works, tending to prove that Muriatic Acid is not compoſed as announced by M. Pachiani*[*].

The galvanic Society engage in experiments to determine the truth of Pachiani's poſition.

As ſoon as the Galvanic Society knew that M. Pachiani, of Piſa, had announced, that he had obtained muriatic acid by depriving water of a portion of its oxigen, their firſt care was to engage in a courſe of experiments both by galvaniſm and electricity, to obtain, if it was poſſible, a confirmation of a diſcovery ſo important to the progreſs of ſcience. The ſociety had a letter communicated to them, which was addreſſed on the 9th of May, 1805, by M. Pachiani, to M. Pignotti; in which he recited the reſults which he had obtained, but without entering into any detail relative to the nature or order of his experiments; they only knew that he uſed the galvanic pile. They therefore determined to make their experiments with the ſame apparatus, in the manner which appeared to them the leaſt likely to produce reſults liable to objections. Two of theſe experiments. which appeared principally worthy of attention, were conducted as follows:

Experiment I.

Deſcription of the apparatus uſed in Exp. 1ſt,

A portion of a new glaſs tube was taken, three inches long, and 0.35 inch in diameter inſide, one of the ends of this tube was cloſed at the lamp; to the other end was united a capillary tube bent in ſuch a manner as to paſs under a jar, and at equal diſtances from the junction of the capillary tube, two points were drawn out at the lamp in the thickneſs of the glaſs, by means of which two bits of gold wire of about 0.02 inch in diameter, and of the ſtandard 0.976 of purity, were inſerted in the tube, at a ſmall diſtance from its lower extremity, and diſpoſed ſo as not to touch each other, or bear againſt the ſides of the tube; theſe points of the glaſs were then cloſed at the lamp. The tube and its capillary prolongation were filled with diſtilled water, whoſe purity had been proved by nitrate of ſilver; the whole was faſtened by wax on a ſmall piece of glaſs, placed in the midſt of an horizontal galvanic pile of fifty-two double ſquare plates of 4.25 inches at each ſide. Theſe plates were ſeparated by pieces of leather, which formed between each other diviſions, which were filled with very pure ſand, moiſtened by a ſolution of muriate of ſoda. The capillary tube was paſſed beneath the water of a ciſtern, with its extremity under the mouth of a jar filled with water. The two wires of gold being made to communicate with the two poles of the pile, its activity was immediately exhibited by the diſengagement of gas in chains of bubbles very apparent, parting from the extremity of each of the gold wires, but in a more conſiderable quantity from that correſponding with the copper pole. This activity of diſengagement of gas continued without interruption from the eighth of Thermidor, to the 23d of the ſame month, on which day, after the pile being moiſtened with ſolution of muriate of ſoda, it ſtopped for ſome time: it ſoon however recommenced, and always did ſo after any ſuſpenſion. Its activity was immediately renewed by agitating the wires which communicated with the poles of the pile. The activity of the pile was conſtantly greateſt at four o'clock in the afternoon; and immediately afterwards it began to diminiſh. On the 11th of Fructidor the apparatus was taken aſunder, after continuing for thirty-four ſucceſſive days in continual action. The water was then diminiſhed to half its original volume, but had not loſt any of its limpidity.

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[*] *Journal de Phyſique*, Tom. LXI. p. 281.

The ſand of the pile moiſtened with ſolution of muriate of ſoda: gas immediately diſengaged on its completion,

the activity of the diſengagement of gas renewed by agitating the wires. This activity always greateſt at four in the afternoon.

The apparatus ſeparated after continuing in action thirty-four days.

The extremities of the gold wires, from whence the gas had proceeded, were oxided, but that moſt perceptibly which communicated with the zinc pole of the pile. The whole gas obtained and collected during the experiment, was 793 cubic centimetres (1200 inches). The liquor remaining in the tube was carefully examined; it produced no ſenſation of taſte on the tongue, had no action whatſoever on the tincture of turnſol and of fernambuc, and did not produce the leaſt cloudineſs in the ſolution of nitrate of ſilver.

The water in the tube diminiſhed one half, the gold wires oxided, that next the zinc moſt, 793 cubic centimeters of gas collected.

The remaining liquor in the tube has no taſte, has no action on turnſole, and is not affected by ſolution of nitrate of ſilver.

The gas produced by the action of the pile was then examined. After having introduced one meaſure of it into the eudiometer of Fontana, an equal meaſure of nitrous gas, made purpoſely for the proof, was added; an abſorption of ſeventy-ſeven two hundredth parts took place in the volume of the two meaſures.

The gas produced is tried by Fontanas's eudiometer, an abſorption of ſeventy-ſeven two-hundredths takes place.

In order to aſcertain whether all the oxigen which the gas contained had entered into combination in this abſorption, a ſecond meaſure of nitrous gas was introduced into the eudiometer; but the gas did not experience any diminution of its volume. The quantity of oxigen which the abſorption produced by the introduction of the firſt meaſure of nitrous gas might indicate, was attempted to be valued by a compariſon with atmoſpheric air eſſayed in the ſame manner; for which purpoſe one meaſure of atmoſpheric air was introduced, and an equal quantity of nitrous gas added; an abſorption of fifty-five two hundredth parts took place in the volume of the two meaſures. In conſidering this abſorption as the effect of the combination of nitrous gas with the volume of oxigen gas, correſponding to the 0,22 parts, which atmoſpheric air contains, it may be concluded, that the abſorption of ſeventy-ſeven two hundredth parts, produced with the gas of the pile, repreſented proportionally the combination of the ſame quantity of nitrous gas with a little leſs than 0,31 parts of the oxigen. It was then obſerved that the meaſures of gas having been introduced ſeparately and ſucceſſively into the eudiometer, it might happen that they were not ſufficiently well mixed, and that conſequently the abſorption was not as compleat as it might be. It was thought that it might be better to paſs the gaſes in ſeparate meaſures under a jar, and then to introduce the whole volume together into the eudiometer. The former experiments having been repeated in this manner, an abſorption took place between the gas of the pile and the nitrous gas, of ninety-two two hundredth parts in the volume of the two gaſes, in place of ſeventy-ſeven reſulting from the ſame proof, by the firſt method; and with the atmoſpheric air and the nitrous gas the abſorption was ſixty-eight two hundredth parts inſtead of fifty-five. There reſults then from this, according to the ſame ratio of the 0,22 parts of oxigen contained in the atmoſpheric air, a proportional indication of about 0,30 parts of this gas contained in that of the pile. It was again proved with the eudiometer of Volta, by introducing a ſingle meaſure into it, through which the electric ſpark was made to paſs; the proof was afterwards repeated ſucceſſively.

The quantity of oxigen contained in the gas valued at 0,31 parts.

The proportion of the oxigen after more exact trials is more accurately valued at 0,30 of the gas.

The gas farther proved by Volta's eudiometer gives the ſame reſult of 0,30 oxigen.

ſucceſſively, on two, three, and four meaſures, and always the abſorption reſulting from the inflammation by the electric ſpark, gave the ſame indication of about 0,30 parts of oxigen.

Experiment II.

Apparatus uſed in ſecond experiment deſcribed.
The ſand is moiſtened with river water containing 1 ſixtieth nitric acid.
The gold wire is diſſolved and precipitated,

very little gas produced, the water in the tube is not diminiſhed 1-fiftieth.

Two grammes (31 gr.) of diſtilled water were put into a glaſs tube bent into the form of a ſyphon; two wires of gold of commerce of about 0,008 inch in diameter, paſſing into the water at about 0,024 inch diſtance from each other, were incloſed in this tube; the tube was then placed upon an horizontal pile of fifty double plates, of about $3\frac{1}{4}$ inches in each ſide. The intervals between them were filled up with dry ſand, and then moiſtened with river water acidulated with about $\frac{1}{60}$ of nitric acid. The wires of gold having been placed in communication with the two poles of the pile, the water in the tube aſſumed in the firſt day a reddiſh brown tint at the ſide of the copper pole, and the wire which paſſed to that part was covered with a coat of oxide of gold of a deep brown colour. The wire which communicated with the zinc pole did not aſſume the ſame tint; the gold of the wire was diſſolved by degrees, and was precipitated together with a part of the ſilver. This precipitate exhibited with a magnifying lens, over almoſt the whole length of the tube, cryſtals in needles. The wire correſponding to the zinc pole was entirely deprived of the gold which covered it, and then only conſiſted of a thread of ſilver of extreme tenuity. But very little gas was diſcharged from either extremities of the wires. The water was not diminiſhed a fiftieth part of its volume.

The pile continued in action forty days.
Indicates by the electrometer an intenſity of 840°.
The remaining liquor in the tube ſhews no trace of acidity by any of the reagents, and has a metallic taſte.

The pile continued in activity 40 days from the 28th of Meſſidor to the 8th of Fructidor. It indicated then on the laſt day by the electrometer (ſimplified by one of the Members of the Galvanic Society, from that conſtructed in Germany, deſcribed in the *Journal de Phyſique* for the month Meſſidor, an. 13,) an intenſity of 840 degrees. The liquor remaining did not afford, by any of the different reagents, the leaſt trace of oxidity; a metallic taſte was alone perceptible in it.

The galvanic ſociety, in examining chiefly the reſults of the firſt experiment, as correſponding more particularly with the fact announced by M. Pachiani, have conſidered, that in allowing for the ſmall quantity of oxigen which had cauſed the oxidation of the extremities of the gold wires, the whole quantity of the oxigen contained by the gas of the pile may be valued in a very near approximation, at 0,31 of its volume; and it is very nearly in this ſame proportion that oxigen gas enters into the formation of water; it was thought that it might thence be concluded, that the whole effect of the galvanic pile, during the entire continuation of the experiment, had been the decompoſition of a part of the water employed, and the ſeparation, in their ſtate of purity, of the oxigen and hidrogen gaſes which compoſed it. The Society then are of opinion, that M. Pachiani was deceived as to the nature of the acid which he announced that he obtained, and that this acid* might have been produced by ſome animal or vegetable ſubſtance employed in the apparatus. The Society does not heſitate to declare, that with the apparatus which they uſed in preference, (as being the moſt ſimple, and the leaſt liable to the influence of other matters,) they do not think it poſſible to effect any thing by the action of the galvanic pile, but a decompoſition more or leſs great, of the water uſed in the experiment.

The Society think the action of their pile only decompoſed the water into oxigen and hydrogen gaſes, and that Pachiani is miſtaken, and that the acid found in his experiment was produced by other means than thoſe he announced, and that it is not poſſible to effect any thing by the galvanic pile in water but its decompoſition.

VIII.

Account of an Ancient Geographical Tablet in the Muſeum of Cardinal Borgia, from † a Memoir preſented to the Academy of Gottingen, by PROFESSOR HEEVEN.

IN the Muſeum of Cardinal Borgia there is depoſited an ancient geographical tablet, from which an engraving has

The tablet was found in the muſeum of Cardinal Borgia;

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* M. Giobert, in Van Mon's Journal, pretends, that the acids and ſalts uſed in the pile *circulate along the wires*, and paſs into any liquor into which they are conducted; which does not appear probable.—B.

The galvanic apparatus uſed in theſe experiments is called a pile through the whole of the French paper, though from its horizontal poſition the appellation does not ſeem very proper. *Trans.*

† This memoir is entitled, " explicatio planiglobii orbis terrarum faciem exhibens, ante medium ſec. xv. ſumma arte confecti; agitantur ſimul de hiſtoria mapparum geographicarum recte inſtruenda conſilia.

The deſign is two feet in diameter, and done in coloured enamel,

limits of the countries not marked,

accounts inſerted in it of various remarkable things.

Why ſuppoſed to have been made in the firſt half of the fifteenth century.

It is the oldeſt geographical deſign, except the chart of Sanudo.

Sweden called Magna Gothia in it, Denmark omitted, Lithuanians called Pagans,

been made, one of the impreſſions of which is in the poſſeſſion of the author of the Memoir. This remarkable monument is not a chart drawn by the pen, but a round tablet, of which the deſign occupies a ſpace about two feet in diameter, on which the hemiſphere, known at that time, is repreſented in coloured enamel, like a round ſurface. The countries and the places are marked by their proper names, but the limits of the countries are not traced; the mountains, the rivers, the people, and all the things remarkable (as the animals, the battles, the caravans, the bazars, the camps, the wandering tribes, &c.) are repreſented and explained on it by inſcriptions in the latin tongue, but written in German characters. It may be conceived from this firſt view how intereſting this monument is, and alſo with what art it is executed, ſo that it is impoſſible to ſuppoſe, that it was made for the uſe of a private perſon. Its date is not mentioned, but it may be determined with certainty that it was conſtructed in the firſt half of the fifteenth century. In reality, the moſt recent event marked on it is the victory of Tamerlane over Bajazet in 1402; there is no mention of the taking of Conſtantinople, or the leaſt trace of any of the diſcoveries of the Portugueſe. Of the geographical charts known at preſent, that of *Marino Sanudo* at the commencement of the fourteenth century is the only one certainly more ancient; but that of *Andre Bianco* of 1436, which *Formaleoni* has made known, is very nearly of the ſame time as this monument. No general ſource of information can be diſcovered by which the author of the tablet has been aſſiſted. It is not made according to documents from Ptolemy; it more follows thoſe of the Arabians, eſpecially with regard to Africa; of the names which are found in the works of *Marco Paolo*, and the other more ancient travellers in Aſia, only ſome are ſeen on that part of the world. The extent of Europe is repreſented as much greater than that of Africa, and at leaſt as large as that of Aſia. The following are ſome of the moſt remarkable particulars of it: Sweden is ſet down under the name of *Magna Gothia*, and Denmark is wanting. In Pruſſia, the ſeat of the wars of the Teutonic order with the Lithuanians, repreſented with this inſcription: *Hic ſunt conſinia paganorum et chriſtianorum, qui in Pruſia adinvicem continuo bellant.* It may be perceived by this, that the Lithuanians were therein conſidered as Pagans, although chriſtianity was introduced among them before this period. Ruſſia appears under the denomination of Tartary, and near the Caſpian Sea and the ſea of Aſof, are repreſented the famous Bazars of thoſe times. England and Scotland appear at the border, but there is no more room for Ireland. Africa exhibits none of the diſcoveries of the Portugueſe, but the northern half of it was known to the author as far as Soudan. He names not only the villages along the coaſt, but he moreover knows that the inhabitants of mount Atlas, the people of Barbary are at war with the Saracens. Near theſe mountains is inſcribed, *In illis montanis habitant plures principes et reges, et habitant continuo in tentoriis, et præliantur continuo contra Saracenos, et contra juxta caſtra et civitates* *. In Egypt the junction of the grand caravan of Mecca is marked, and not only the names of the deſerts of ſand are inſerted, but thoſe alſo of the places moſt important to commerce, as Tagaza, Ganuſia, &c.

Ruſſia called Tartary in it. Places of the Bazars repreſented.

It contains none of the diſcoveries of the Portugueſe in Africa.

The junction of the Caravans of Mecca noted in Egypt.

The kingdom of Preſter John extends in Nubia *ab oſtio gandis* (Cape Gardefan) *usque ad fluvium auri*. Bianco likewiſe ſets down the kingdom of Preſter John in Africa in the ſame manner, ſo that the Portugueſe are not the firſt who have thus deſcribed it. Aſia does not preſent fewer ſingularities. In Aſia Minor the camps of the Tartars are repreſented; *Tartaria reges maxima, quu Tartari cum ſuis jumentis et bobus excurrunt, civitatem ex multis tentoriis et carutes ſituant.* India is divided into *India ſuperior*, where the body of St. Thomas is found, and many chriſtian kingdoms, and *India interior, in qua Cathai civitas et magnificanis Imperatoris Tartarorum ſedes.* China is likewiſe inſerted in it, and its capital Cambalck (Cambalu Pekin) is alſo named. On the frontiers of little Bucharia at Organti, (Urgang)

Extent of Preſter John's kingdom ſet down.

Camps of the Tartars in Aſia Minor marked.

the diviſions of India, and poſition of Cathai inſerted, and China with its capital Cambalk (Pekin.)

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* The Latin of the inſcriptions in this paper is not very correct, *continuo* is uſed in them for *ſemper*, and *juxta* for *vicina*, the word *ſituant* is alſo improper, and ſome others, but theſe circumſtances perhaps only mark more ſtrongly the authenticity of the account. In the inſcription relative to India interior, a ſmall alteration has been made from the memoir in this tranſlation. The word *magnificanis* has been formed from *magni canis* in the memoir, which being ſo printed, evidently was an error.—B.

The route of the caravans to Cathai; the country of Gog and Magog; and the ſite of Paradiſe.

de Organti ad Carthagium vadunt cameli in quatuor menſibus, the caravans going and returning to Cathai are repreſented. On the eaſtern border the country of Gog and *Magog* is ſet down, and finally *locus deliciarum* or paradiſe.

IX.

Analyſis of Birdlime. By M. Buillon Lagrange.*

Sect. I.

The Origin and Preparation of Birdlime.

Various opinions on the nature of birdlime;

THE ſubſtance known by the name and appellation of *birdlime,* has been claſſed among the immediate productions of vegetables.

M. Fourcroy,

M. Fourcroy was the firſt perſon who conſidered this matter to be glutinous: he has deſcribed it as a ſpecies in his "*Syſteme des Connoiſſances Chimiques,*" Vol. VIII. p. 306.

Birdlime, according to this chemiſt, may be made of the berries of miſtletoe, or of the tender bark of the holly, and ſeveral other kinds of trees, macerated in water. Although this ſubſtance appears to have been hitherto not examined with ſufficient accuracy, many qualities have been diſcovered in it analogous to thoſe of gluten,

Excepting a few chemical properties, mentioned in my "*Manuel d'un Cours de Chimie,*" third edition, I have never found in any work the leaſt elucidation of the nature of this ſingular ſubſtance.

M. Chaptal,

M. Chaptal, in his "*Elemens de Chimie,*" ſpeaks only of its preparation. As the method preſcribed by this chemiſt is nothing different from that in the "*Materia Medica*" of Geoffroy, and in the "*Dictionary*" of Valmont de Bomare, I ſhall quote the article itſelf.

Geoffroy, Valmont de Bomare.

Ancient mode of preparing it, by boiling and pounding the berries of miſtletoe;

"The ancients made uſe of the berries of the miſtletoe of oak in the preparation of birdlime. The berries being firſt boiled in water, were pounded, and the hot water was then poured off, in order to carry away the ſeeds and rhind. At preſent birdlime made of the bark of holly is preferred. The middle bark is made choice of, as being the moſt tender and green: this is placed in a pit to rot, after which it is pounded in mortars until it becomes a paſte, and is then waſhed and cleanſed with water. This ſubſtance has been conſidered as diſcuſſive and emollient, when applied outwardly."

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* Annales de Chimie, Vol. LVI.

at preſent it is made from holly bark rotted and pounded.

It is already known that the miſtletoe of oak is employed in ſeveral pharmaceutical preparations; as, the univerſal water, the antiſpaſmodic powder, Guttet's powder, &c.

Miſtletoe of oak uſed in pharmacy.

In England, according to Geoffroy, birdlime is made of the bark of holly. He ſays, the bark is boiled in water ſeven or eight hours, till it becomes ſoft and tender. This is laid in maſſes in the earth, and covered with ſtones, placing one layer over another—the water having been previouſly drained from the bark. In this ſtate it is left to ferment and rot, during a fortnight or three weeks, in which time it changes to a kind of mucilage. It is then taken from the pit, pounded in mortars till reduced to a paſte, waſhed in river water, and kneaded till freed from all extraneous matters. The paſte is left in earthen veſſels during four or five days, to ferment and purify itſelf. It is afterwards put into proper veſſels, and thus becomes an article of commerce.

Engliſh mode of making birdlime: Bark of holly is boiled, rotted, pounded, waſhed, and kneaded.

This mode of preparation is not univerſally followed, as every county has its peculiar way: there are even thoſe who make a ſecret of the proceſs.

At Nogent-le-Rotrou birdlime is manufactured by cutting in ſmall pieces the ſecond bark of the holly, fermenting them in a cool place during a fortnight, and then boiling them in water, which is afterwards evaporated.

Method purſued at Nogent-le-Rotrou; firſt rotting and then boiling:

At Commerci and its environs birdlime is obtained from ſeveral ſhrubs, as the holly, the wild vine (*viburnum lantana,* Lin.) and the miſtletoe of every ſpecies.

At Commerci.

The beſt is that made from the prickly holly, which is greeniſh; that obtained from the *viburnum lantana* is of a yellowiſh tint. In uſing this latter, the epidermus is rejected, and only the ſecond bark employed.

Beſt ſort from the prickly holly.

The birdlime which I uſed in my experiments was made from the ſecond bark of holly; and on comparing it carefully with ſome which had been ſent me from Commerci, I found there was no apparent difference between them. I thought this precaution eſſential to obtaining greater preciſion in my analyſis. It is well known that the birdlime of commerce is never in a pure ſtate; it is frequently a compoſition of vege-

table

Birdlime of commerce frequently adulterated.

table and animal matter; ſometimes it is even adulterated with turpentine, oil, vinegar, &c. It was therefore neceſſary that I ſhould be certain as to its purity; and by the following mode, which I adopted, I obtained birdlime of the very beſt quality.

The author's preparation of birdlime.

Having procured a ſufficient quantity of the ſecond bark of holly, I bruiſed it well, and boiled it in water for four or five hours: the water being poured off, I depoſited the bark in pits, in earthen pans, where it remained till rotten, or at leaſt till it became viſcous, moiſtening it from time to time with a little water. When it had obtained a proper degree of fermentation, it was cleanſed, by waſhing, from all heterogeneous matters.

SECT. II.

Chemical and phyſical Characteriſtics of Birdlime.

Characteriſtics.

Birdlime is of a greeniſh colour, and of a ſour flavour: it is gluey, ſtringy, and tenacious. Its ſmell reſembles that of linſeed oil.

It becomes dry and brittle by expoſure to the air;

Spread on a glaſs plate, and expoſed for ſome time to the action of air and light, it dries, and becomes brown in colour, being no longer viſcous. When quite dry, it may be reduced to powder, in which ſtate it is totally diveſted of its adheſive qualities, and only recovers them on the addition of water.

but viſcid again when wetted.

Birdlime reddens tincture of turnſole.

By moderate heat it fuſes.

When gently heated in a porcelain veſſel it melts, but does not become very liquid; it ſwells in bubbles, which float upon the ſurface. This kind of fuſion produces ſmall black grains, which render the birdlime grumous: it produces a ſmell very ſimilar to that obtained from animal oils, on raiſing their temperature.

If this fuſion be continued for ſome time, the birdlime aſſumes a browniſh colour; but recovers its proper characteriſtics on cooling.

Strong heat inflames it;

Placed on red-hot coals, it burns with a briſk flame, and creates a great deal of ſmoke.

Heated in a crucible of platina, it takes fire when the crucible is red-hot; produces a lively flame, which riſes to about two decimetres, accompanied with a conſiderable quantity of ſmoke, which eaſily attaches to the chimney: this combuſtion always takes place, although the crucible be taken out of the fire.

leaving an alkaline reſidue.

A whitiſh reſidue is left, which is very alkaline and partly ſoluble in water. Re-agents demonſtrate the preſence of ſulphate and muriate of potaſh.

Experiments on the reſidue.

That part which water could not take up, on being put into muriatic acid was diſſolved with ſome efferveſcence.

It contains ſoluble ſalt and carbonate of lime, and alumine and iron,

This liquid is copiouſly precipitated by the oxalate of ammonia; pruſſiate of potaſh gives a blue precipitate. That produced by ammonia is of a paſty conſiſtence, partly ſoluble in cauſtic potaſh: whence may be inferred that the reſidue, independent of the ſalts ſoluble by water, is compoſed of the carbonates of lime and of alumine, with a ſmall portion of iron.

Water has little action on birdlime;

Water has very little influence on birdlime. On boiling, the matter does not completely diſſolve, but acquires merely a ſmall increaſe of fluidity, which it loſes in cooling, and reſumes its primitive conſiſtency.

This water obtains no colour; its flavour is at firſt inſipid, afterwards ſour, and it reddens tincture of turnſole.

Evaporated to the conſiſtency of a ſyrup, it becomes coloured, with a mucilaginous appearance, which may be ſeparated by alcohol.

It takes up ſome mucilage.

The action of water, therefore, is confined to the ſolution of a mucilaginous ſubſtance, with a ſmall portion of the extractive matter.

Cauſtic potaſh combines with birdlime and gives out ammonia.

It is not thus with cauſtic potaſh. Its concentrated ſolution forms at once with birdlime a whitiſh magma, which turns brown on evaporation, with a ſeparation of ammonia.

This compoſition is leſs viſcid; it acquires a great degree of hardneſs by expoſure to the air; and its ſmell and taſte are ſimilar to thoſe of ſoap.

The compound has the habitudes of ſoap.

It is chiefly ſoluble in water and alcohol, there remaining but a few vegetable dregs. Theſe ſolutions are affected by ſtrong acids; but theſe kinds of decompoſitions preſent no new phenomena to thoſe obtained with a ſolution of ſoap.

Weak acids ſoften and partly diſſolve birdlime.

The moſt feeble acids ſoften birdlime, and partly diſſolve it: when concentrated, they act in a different manner.

Sulphuric acid charrs it.

Sulphuric acid renders it black and charry: by adding powdered lime, ſo as to form a thick magma, a ſeparation of acetic

tic acid and ammonia is procured. There can be no doubt but that in addition to the acetic acid naturally preſent in birdlime, more is produced by the action of the ſulphuric acid.

Hot nitric acid decompoſes it, and forms a kind of wax:

Nitric acid, whilſt cold, has very little power over birdlime: but on increaſing its temperature, the mixture turns yellow, diſſolves, and as evaporation advances ſwells conſiderably, leaving at laſt a hard brittle maſs. This maſs, when a ſecond time ſubmitted to the action of nitric acid, is diſſolved, a part being converted into malic and oxalic acids. By continuing the evaporation a yellow matter is obtained, eaſily friable, yielding to the preſſure of the fingers like wax, with a kind of elaſticity, and melting by means of a gentle heat.

which forms ſoap with potaſh.

Potaſh combines with this matter, changes its colour from yellow to brown, and forms perfect ſoap.

Alcohol partially diſſolves it,

Alcohol partially diſſolves it, and becomes yellowiſh; its tranſparency is diminiſhed by the addition of water.

On evaporating the alcohol to dryneſs, there remains a yellow matter diveſted of the greaſy appearance, which yields a ſweet odour in burning.

Cold muriatic acid has little action on birdlime.

Cold muriatic acid has no action upon birdlime: when heated it turns it black.

Oxigenated m. acid alters birdlime conſiderably.

Oxigenated muriatic acid operates differently. Either by mixing the gas with the water containing the liquid birdlime, or by ſhaking it in a bottle with the acid in a very concentrated ſtate, the following phenomena were equally obſerved;

The birdlime quickly loſt its colour, and became white; it was no longer viſcid, but divided into hard compact portions, containing in their centre a quantity of birdlime, which the oxigenating principle had not reached. This non-oxigenation may be attributed to the difficulty there is of preſerving this ſubſtance in its liquefied ſtate in hot water, whereby the operation of the acid is confined to its exterior ſurface.

Characteriſtics of oxigenated birdlime.

The characteriſtics of oxigenated birdlime are—

1. It is capable of being reduced to powder.

2. It is inſoluble in water, even when heated.

3. It does not become liquefied at a high temperature.

4. It will not turn yellow, nor will it form a reſin with nitric acid.

Acetous acid ſoftens birdlime,
&c.

Acetous acid ſoftens birdlime and diſſolves a certain quantity; the liquor acquires a yellow colour; its taſte is inſipid.

Carbonate of potaſh produces no precipitate; evaporation gives a reſinous reſiduum, which cannot, however, be brought to a ſtate of perfect dryneſs.

Birdlime reduces metallic oxides.

Certain metallic oxides are eaſily reduced on being heated with birdlime.

Semi vitreous oxide of lead incorporates with birdlime.

Semi-vitreous oxide of lead aſſumes a grey colour, diſſolves, and forms with the birdlime an emplaſtic maſs.

Boiling alcohol diſſolves birdlime,

Alcohol at 40 degrees, and boiling, diſſolves birdlime ſo long as it is kept hot; it is clear, and of a tranſparent yellow colour; but in proportion as the liquor cools, it becomes turbid.

and lets fall a kind of wax by cooling;

A yellow matter may be ſeparated by filtering, which is much ſofter than the original maſs; melts in a moderate heat, diffuſing a ſmell analogous to that of wax, of which it ſeems to poſſeſs all the qualities.

retaining a reſin.

The filtered liquor is bitter, nauſeous, and acid; precipitating in water, and leaving on evaporation a ſubſtance ſimilar to reſin.

Sulphuric ether the true ſolvent of birdlime.

Sulphuric ether may be conſidered as the true ſolvent of birdlime: its action on this ſubſtance is rapid, firſt dividing it, and then diſſolving it nearly *in toto*, there remaining only a few vegetable dregs. The liquor acquires a greeniſh yellow colour, and ſtrongly reddens turnſole. On adding a little water the mixture thickens, and the ether ſwims on the top; but if a ſufficient quantity of water be poured in to diſſolve the ether, a body of oil is formed on the ſurface, bearing a conſiderable analogy to that of lintſeed: this may be converted, by the aſſiſtance of the ſemi-vitreous oxide of lead, into an emplaſtic maſs.

Water ſeparates oil from the ſolution.

Ether by evaporation gives a greaſy ſubſtance.

By evaporating the ſolution of birdlime in ether, a greaſy ſubſtance is obtained, of a yellow colour, and of the ſoftneſs of wax.

Concluſion.

From the foregoing obſervations it will be perceived how little analogy exiſts between birdlime and gluten.

General properties of birdlime.

A ſimple compariſon will be ſufficient to deſignate the place it ought to occupy among vegetable productions.

Birdlime is viſcid, elaſtic, dries a little in the air, by expoſure to which it becomes brown; but is not rendered brittle and irrecoverable like gluten.

It melts in the fire, ſwells, and burns with a vivid blaze; but does not diffuſe that animal odour which is to be obſerved in gluten.

Water will not diſſolve birdlime; it merely imbibes the mucilage, the extractive matter, and the acetic acid.

Alcalies diſſolve it; when concentrated, they convert it into ſoap.

Dilute acids ſoften and partially diſſolve birdlime.

Concentrated ſulphuric acid renders it black and charry.

Nitric acid turns its colour to yellow, converting the ſubſtance partly to malic and oxalic acids, and partly to reſin and wax.

Oxigenated muriatic acid renders it white and ſolid, conſtituting *oxigenated birdlime.*

Alcohol exerts but little action upon birdlime; it diſſolves the reſin and deſtroys the acid.

Laſtly, ſulphuric ether diſſolves it entirely.

Recapitulation of the points wherein it differs from gluten.

Birdlime, therefore, differs from gluten,

1ſt. In the acetous acid which exiſts in it.

2d. In being very ſlightly animalized.

3d. In the mucilage and extractive matter which may be obtained from it.

4th. In the great quantity of reſin which may be obtained from it by means of nitric acid.

5th. In its ſolubility in ether.

X.

Method of purifying Oil. *By M.* CURAUDEAU.*

The purification of oil conſiſts in its clarification.

THE purification of oils for combuſtion conſiſts ſolely in their clarification: It is only ſince Argand's lamps have come into common uſe that this ſubject has received much attention.

There are many proceſſes for the purification of oils, but all are not equally good; and thoſe who ſell purified oil make a ſecret of the method of purification.

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* From Cours complet d'Agriculture, Tome XII.

However, as the art of purifying oils ought to be known by thoſe who manufacture them, the proceſſes, which are conſidered the moſt economical and ſimple, ſhall be here mentioned; by which information they will be able to obtain that profit which thoſe now make who follow this ſpecies of induſtry after them.

The proceſs for the purification of oil by ſulphuric acid, which follows, is little different from that publiſhed by Thenard.

Proceſs by ſulphuric acid deſcribed.

To one hundred parts of rape oil one part of ſulphuric acid is to be added, diluted with ſix times its weight of water; the mixture ſhould be ſtrongly agitated, and as ſoon as this is completely finiſhed, it is left ſtill till the oil becomes clear; when it is perfectly clear the purification is effected.

Sulphuric acid and water added to the oil; the mixture ſtrongly agitated, and left to ſettle.

There remains at the bottom of the veſſel an acid liquor ſomewhat coloured: the oil is to be ſeparated from the ſediment; and in order to be certain that no acid is retained by the oil, ſome ounces of powdered chalk is to be added; the mixture ſhould then be ſhaken, and the oil again left quiet to ſettle.

The oil ſeparated from the ſediment; powdered chalk added; agitated again, and left to ſettle.

The action of the ſulphuric acid in this proceſs conſiſts in depriving the oil of all its humidity, although it is itſelf mixed with water, and in ſeparating from it a mucoſo-extractive ſubſtance, the preſence of which diminiſhes the energy of the combuſtion of the oil, covers the wick with charcoal, and produces much ſmoke: It is then on the abſtraction of theſe principles foreign to the oil, that its quality of giving a good light depends.

The acid in this proceſs ſeparates the mucoſo-extractive matter from the oil, which injures its combuſtion.

Another Method.

The next proceſs to be deſcribed has been followed by ſome manufacturers, who have had good ſucceſs with it.

Proceſs by flour and water.

To one hundred parts of rape-ſeed oil ten parts of water are to be put, to which one part of wheaten flour has been added; the mixture is to be well agitated, and then to be heated until all the water added has been evaporated, or, more properly, until the oil has ceaſed to have any union with the ſubſtances which it held in ſuſpenſion: In this ſtate it becomes purified; and at the end of twenty-four hours it is very clear, and does not differ at all in quality from that prepared by ſulphuric acid.

Flour and water added to the oil; the whole well agitated, and heated till a ſeparation of the contained matters enſues; in 24 hours after this it is clear.

The heat ſhould be applied gradually, and ſhould not exceed 80° Reaumur.

In the practice of this laſt proceſs, care ſhould be taken to heat the oil gradually, and not to raiſe its temperature above 80 degrees of. Reaumurs thermometer. (212 Fahrenheit) This heat is ſufficient to effect the coction of the flour, and of the mucoſo-extractive matter contained by the oil; a greater degree of heat would colour the oil, and deprive it of the appearance moſt favourable to its ſale.

M. Curaudeau led to this proceſs by obſerving the ſeparation of white ſauce into two ſubſtances when too much done.

M. Curaudeau was led to this proceſs by an obſervation, which every one may likewiſe make. It is well known that the ſauce called melted butter, when too much boiled is ſeparated into two parts, one which is thick and occupies the bottom of the veſſel, while the other part is clear and floats above the firſt: The lower ſubſtance is the caſeous part of the butter united to the flour that has been added to the ſauce, and which the action of the fire has ſeparated from the oil; The upper ſubſtance is the butter deprived of all foreign matter; and in this ſtate it may be called *purified butter*.

XI.

On a peculiar Fluctuation of the River Dordogne, called the Maſcaret. By M. LAGRAVE SORBIE *.

The maſcaret takes place only when the waters are low.

THE peculiar movement of the waters of the River Dordogne, which is called the *Maſcaret*, takes place twice each day in the ſummer time, *when the waters are low*, which is an eſſential condition. A ſimilar motion alſo takes place on the river Amazons according to the report of M. de la Condamine, when it is named the Pororoca; the ſame is alſo perceived at the Orcades, off the north of Scotland, according this author: and M. Sorbie has ſeen accounts in the publications of ſome voyagers of its likewiſe occuring in ſome of the rivers of Hudſon's bay, and alſo in the Miſſiſſipi.

The Pororoca of the Amazons ſimilar to it, a like occurrence at the Orcades, and in the rivers of Hudſons bay, and in the Miſſiſſipi.

It is not ſurpriſing that this phenomenon does not happen in all rivers; it is not always ſeen even in the Dordogne. From the moſt exact obſervation, if the ſummer is not dry, and that the waters are not low to a certain degree the Maſcaret does not appear, It rarely occurs in winter; it however ſometimes takes place during very hard froſts, when the cold has diminiſhed the waters by the formation of much ice; but this happens very ſeldom, and has never been more than three times, in ſeveral ages.

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* Journal de Phyſique, LXI. 286.

There is a *maximum* of depreſſion in the waters neceſſary to its appearance; Wherefore the mariners in the neighbourhood of Bourdeaux are in the habit of talking of it ſomewhat in this manner, "The waters have fallen ſo much, the tide will encreaſe to day to ſuch a height, we ſhall have a Maſcaret", and they load their barks accordingly, and take precautions to avoid it. The manœuvres of theſe mariners have cauſed ſome naturaliſts in the vicinity of Bourdeaux to obſerve long ſince, that this phenomenon muſt depend on a natural cauſe, connected with the bed of the river, ſince theſe men can foretel, without being ſcarcely ever miſtaken, by the depreſſion of the water, whether the Maſcaret ſhall appear or not, although ſometimes it has not appeared before for ſome years, becauſe the rains have prevented the waters from diminiſhing to the neceſſary degree.

The mariners foretel the Maſcaret from the lowneſs of the river,

No one has yet tried to explain the cauſe of this ſingular fact, not even M. Condamine, or if there be any works on the ſubject, they are unknown to the author though his ſtudies have been particularly directed in the line where ſuch information might occur, and he has read much. In order to enable others to account for the facts, he mentions thoſe which relate to it ſuch as he has himſelf ſeen, and ſuch as he has been told have been witneſſed for ſeveral ages.

no account of the cauſe of the Maſcaret yet publiſhed.

In the ſummer, or, more properly ſpeaking, when the waters are low, there appears at a little diſtance from the junction of the Dordogne with the Garonne, or at Bec d'Ambes, an accumulation of water, like a promontory, on the ſhore, which is from the thickneſs of a ton to that of a ſmall houſe, and which rolls along with ſuch velocity that no horſe, whatever might be his ſpeed, could keep up with it. It follows the direction of the ſhore, and makes a moſt frightful noiſe. The horſes and oxen, which feed in the meadows near the river, run away with their utmoſt ſpeed exhibiting the greateſt terror; ſo much ſo that they remain trembling a long time after, and cannot be driven back but with much difficulty. The ducks and geeſe have alſo been ſeen to precipitate themſelves into the reeds at its approach, with the greateſt ſpeed and af-

It conſiſts of an accumulation of water, which appears firſt at Bec d'Ambes, and ruſhes up the river with great velocity, and a frightful noiſe, terrifies the cattle, and the water fowl, fright, and lie flat there, without being able to come out. Hard bodies, which lie in the way of the Maſcaret are ſtruck by it with ſuch force, that the piers, built for the uſe of the veſſels, along the ſhore are demoliſhed, and ſome of the ſtones which compoſe them, although very large, are driven away more than fifty paces; the ſtrongeſt trees are torn up by the roots, the barks which it meets are not only ſunk, but broken aſunder, eſpecially if they are near the ſhore, or have any hard body lying beneath them. From the place called St. Andre (See the lower part of Plate IV.) on the river, the Maſcaret forms itſelf into waves which half its breadth as far up as Caverne; there it diſappears for a ſhort time, to appear again between Aſque and Lile like a promontory, and then returns into the form of waves as far on as Terſac; at Terſac it regains its firſt appearance, which it only quits at Vayne; from Vayne it proceeds along the bank as far as Fronſac, the houſe of M. de Richelieu; from Fronſac it occupies the whole breadth of the river, paſſes with a terrifying noiſe before the village of Libourne, throws the road for veſſels belonging to this village into confuſion, and afterwards appears at Geniſac-les Reaux and at Peyreſite with but very little force. The whole paſſes in the courſe of ſeven or eight leagues.

overturns the piers along the river, and drives the large ſtones which compoſe them, more than fifty paces off, tears up large trees by the roots, and ſinks and breaks veſſels in pieces. Above St. Andre it appears in waves, above Aſque is ſeen in its original form; in waves again above Lile, at Terſac it regains its firſt appearance; at Fronſac it occupies the whole breadth of the river, paſſes before Liſbourne with a terrible noiſe, and ceaſes at Peyreſite.

The following is the account, which M. la Condamine gives of the *Pororoca* of the river Amazons, the compariſon of the effects of which with thoſe of the Maſcaret will tend to eſtabliſh the theory of theſe phenomena.

Account of the Pororoca on the Amazons:

In his voyage to the river Amazons, page 193, he relates, that " between Macapa and Cape-Port, where the channel of the river is moſt confined by the iſlands, and eſpecially oppoſite the mouth of the Arawary, which joins the Amazons on the north ſide, the flowing of the ſea exhibits a ſingular phenomenon. During the three days next the full of the new moons, the times of the higheſt tides, the ſea, inſtead of taking almoſt ſix hours to riſe arrives at its greateſt heighth in one or two minutes; it may be conceived that this does not happen quietly; there is heard at a league diſtance a terrible noiſe, which announces the *Pororoca*, which is the name that the Indians of theſe parts give to this frightful flood. In proportion as it approaches the noiſe encreaſes, and ſoon an accumulation of water, like a promontory, appears from 12 to 15 feet high; after that another is ſeen, then a third, and ſometimes a fourth, which follow

its noiſe heard at a league diſtance. It advances in ſeveral waves, each twelve or fifteen feet high, each other cloſely, and which occupy the whole breadth of the channel. Theſe waves advance with a prodigious rapidity, break and overturn every thing which oppoſes them. I have ſeen in ſome places a large extent of land carried away, great trees torn up by the roots, and ravages of all kinds committed; every where that they paſs the banks are ſwept clean; the canoes, the pirogues, and even the barks can only eſcape their fury, by anchoring in deep water. After having examined this phenomenon with attention in different places, I have always remarked that it only takes place, when the riſing flood is engaged in a narrow channel, or meets in its way with a bank of ſand, or a ſhallow place, which occaſions an obſtacle to it; that it was in thoſe places alone that this impetuous and irregular movement of the waters commenced, and that it ceaſed a little beyond the bank, when the channel became deeper, or grew conſiderably wider. It is ſaid that ſomething ſimilar to this happens at the iſles of the Orcades, at the north of Scotland, and at the entrance of the Garonne, (it ſhould be the Dordogne), in the vicinity of Bourdeaux, where the effects of theſe tides, is called a *Maſcaret*."

which ruſh forward with great rapidity, and overturn every thing which oppoſes them, carry away large portions of the land, and tear up trees by the roots.

It only occurs in narrow channels, over ſand banks, or ſhallow places.

It ceaſed where the channel became deeper or wider.

It appears from what has been cited from M. Condamine, that the effects of the Pororoca are almoſt the ſame as thoſe of the Maſcaret. Nevertheleſs there is a marked difference between them in this reſpect, that on the Dordogne, two kinds of floods take place, one which extends over the whole river, and is ſimilar to that which M. Condamine has obſerved, and the other which ranges along the ſhore, rolling more over the depoſits which the waters have left, than in the water itſelf. He ſays poſitively in page 194, that "at one or two leagues a frightful noiſe is heard, which announces the Pororoca; as it approaches the noiſe encreaſes; and ſoon an accumulation of water appears from 12 to 15 feet high, and then another that follows, which occupies the whole breadth of the channel". On the Dordogne the Maſcaret riſes with great noiſe, ſometimes along the coaſt in an elongated accumulation, and ſometimes in the form of frightful waves, which extend over the whole river; when it follows the ſhore it only appears in the re-entring angles, and on the ſand banks, as is deſcribed in the ſketch of the plan of the river, which accompanies this account, and which takes in the whole extent where the effects of the Maſcaret are perceived. The parts covered with

Difference between the Maſcaret and Pororoca.

further particulars of the Maſcaret.

Deſcription of the ſketch of the courſe of the

ſmall

Maſcaret in the river.

ſmall points, indicate the ſand banks where the Maſcaret always commences; the parts occupied by ſmall lines, are the places where the waves occupy the whole breadth of the river. The dotted parts indicate the re-entring angles, where the ſand banks are found which are depoſited by the counter current. It is here principally that the Maſcaret rolls with all its fury over the mud of the river. On the banks the ſalient angles are the places where the Maſcaret quits the ſhore, occupies the whole river, and runs upwards, accompanied by many conſiderable waves, which ſucceed each other, till another re-entring angle occurs, where it again reſumes its firſt form.

It is thus that theſe who dwell in the vicinity of Bourdeaux witneſs without emotion twice each day, when the waters are low, ſo extraordinary a phenomenon, without any one thinking of examining into the cauſe of it, or even of communicating the particulars to naturaliſts.

The tide is the primary cauſe of the Maſcaret.

The courſe of the Dordogne deſcribed, to account for the Maſcaret,

The primary cauſe of this riſing of the water is the ſame as that of the tide in all rivers; and if the Maſcaret occurs on very few rivers, it is becauſe their beds are not formed in a manner neceſſary to produce it, and have not the ſame diſpoſition as the Gironde and Dordogne: they have either too little or too great a current; their waters are not ſufficiently low, or when they are, the tide does not continue long enough; finally the re-entring and ſalient angles are not ſuch as they ought to be. M. Sorbie thinks he could tell before hand whether any river would be liable to ſuch effects, from the form of its plan and the diſpoſition of its bottom; and is of opinion that the cauſe why more rivers are not ſubject to the Maſcaret, depends entirely on the ſhape of their beds, and not on any particularity in their tides. The phyſical cauſe of that on the Dordogne appears very ſimple, M. de la Condamine ſays, that on the Amazons it is always at the narrow parts where it is obſerved. The cauſe is not the ſame on the Dordogne, for there is no narrow parts in almoſt its whole courſe: it is nearly every where very rapid, and of ſmall depth, as all thoſe rivers are which have much current. It forms, as may be ſeen in the plan, many turns and windings; and has few iſles: but at each angle a bank of ſand is depoſited: It deſcends, notwithſtanding theſe windings, almoſt from the eaſt to the north-weſt. As far as Bec d'Ambes, where it unites with the Garonne which is much more powerful than it, and they form together that beautiful arm of the ſea, called the Gironde. The two rivers then deſcend together from Bec d'Ambes to the ſea in a direction from the eaſt to the north-weſt. All the waters which arrive from the arm of the ſea or from the river, advance in a ſtraight line with abundance into the mouth of the Dordogne, inſtead of mounting up the Garonne, which runs almoſt north and ſouth as far up as Bourdeaux. The greateſt part of the waters which are advancing to the Garonne, ought then, when the current has taken its courſe, to run up the Dordogne at the beginning of the flood, ſince its velocity does not allow them time to turn up the Garonne; and thus the water which ought to go to the Garonne, running up the Dordogne, form by their abundance, this effect which Condamine recites: He ſays that "the tides, which uſually take ſix hours to riſe, arrive at their full height in one or two minutes". But on the Dordogne, the tides never come to their higheſt level in near ſo ſhort a time, even when the waters are loweſt; but in one or two minutes they encreaſe conſiderably; which encreaſe is probably cauſed by the waves which arrive almoſt inſtantly; and the flood raiſing their maſſes of water above their natural level, leaves them there to augment the water in the bed of the river in proportion to their bulk. After the Maſcaret has paſſed, the waters of both rivers encreaſe in the ſame gradual manner as thoſe of all other rivers.

ſuppoſed to be cauſed by the waters on their way to the Garonne, taking the ſtraighter courſe up the Dordogne.

M. Sorbie likewiſe thinks, after all, that the tide of the Gironde may be the cauſe of the Maſcaret on the Dordogne, for it pours its waters into the mouth of the Dordogne in almoſt a right line; this arm of the ſea being at leaſt ſix times larger and deeper than the Dordogne, ought at the flood to carry up ſuch an abundance of water, as could not enter into the bed of this river without occaſioning the accumulation of waters deſcribed. The phyſical cauſe then of the Maſcaret is the conſiderable maſs of water which arrives from the Gironde into the mouth of the Dordogne, and the ſmall depth of this river; ſince it is known that in rainy ſeaſons, and when the river is a little encreaſed in ſize, this circumſtance never takes place.

It may alſo be cauſed by the tide of the Gironde ruſhing in a right line into the mouth of the Dordogne, and by the ſhallowneſs of this river,

M. Sorbie remarks in concluſion that the facts related ſhew evidently that the flowing and ebbing of the tides of rivers are different from thoſe of the ſea; that the ebbing and flowing of rivers, are only ſecondary effects of the tides of the ſea; that is to ſay, that the waters of the ſea only form a dam to

remarks on the tides of rivers, ſuppoſed to be cauſed by the tide of the ſea forming a dam acroſs their courſe.

thoſe of the rivers, and that the rivers form by the abundance of their waters, thoſe rapid flood-tides which are obſerved on the great rivers, ſuch as thoſe of the river Amazons, which aſcend from 5 to 100 leagues, thoſe of the Senegal, which advance almoſt as far up, and thoſe of other rivers almoſt equally conſiderable. M. Sorbie thinks that the Maſcaret, or the Pororoca, have altogether the ſame cauſe as the flood-tide of rivers, and though ſome ſlight ſecondary effects occur, ſuch as thoſe related, that all ariſe from the ſame phyſical cauſe.

XII.

Deſcription of a ſecret Lock of ten thouſand Combinations.

W. N.

Diſquiſition upon locks. The common lock.

THE common lock uſually conſiſts of a bolt, which requires a particular inſtrument, called the key, to puſh it backward and forward; and in order that this bolt may be inacceſſible to violation, certain impediments or obſtacles, uſually called wards, are interpoſed between the key-hole and the bolt, which make it difficult to open the lock by any general or common proceſs. The general proceſs for picking a lock, of which the key has not been ſeen, conſiſts in operating upon the bolt by a ſmall bended inſtrument or wire; or elſe by endeavouring to diſcover the poſition of the wards by an unperforated key, on the face of which ſome ſoft or plaſtic matter is lodged. And when this ſituation is once diſcovered, it is not difficult to file away ſo much of the key as ſhall allow it to paſs, or elſe to ſelect, out of a number of ſkeleton keys, one, of which the form ſhall admit of its paſſing through the lock. There are many locks ſo ſituated, as for example in the veſtries of churches and other little frequented places, as to admit of this ſlow operation; but it muſt at the ſame time be allowed, that the Engliſh market preſents locks of a number of different conſtructions, which can neither be picked nor analyzed by the proceſs here mentioned. Nothing is more common, however, than for keys to be entruſted out of the hands of the poſſeſſor, or to be hung up, or caſually laid down or miſlaid. In theſe circumſtances their figure may be taken with

Bolt, key, wards, pick-locks, ſkeleton-keys.

with wax, like the impreſſion of a ſeal, or more ſpeedily by indentation upon a piece of moiſtened paper, or by various other means; and it muſt be admitted, that very little ſkill is required to enlarge the openings of a common key, ſo as to make it paſs the wards of a ſuperior lock.

Secret locks; their ſtructure and imperfection.

Theſe neceſſary and unavoidable imperfections of common locks, have long ago led to the introduction of ſecret locks, which are ſo conſtructed as to require ſome particular manipulation in opening them; ſuch as that the key ſhould be turned twice round, or that it ſhould be turned through a certain ſpace in one direction, and then back again; or that it ſhould act upon ſome delicately reſiſting piece, very likely to be diſregarded by an uninſtructed poſſeſſor of the key; or that a number of viſible parts ſhould be placed in ſome determined order, before the common proceſs of opening, either with or without a key, can take place. Upon all theſe contrivances one general remark may be made, namely, that the poſſeſſor muſt always in perſon open his own lock; for if this be to be done by the mere practice of a ſecret without a key, his cabinet becomes for ever open to him who, by communication or otherwiſe, ſhall poſſeſs that ſecret; and if a key be uſed, his lock, as to that perſon, becomes as ſubject to violation as a common lock.

Methods of violating them.

In the mechanical conſideration of a ſecret lock, we may ſuppoſe the conſtruction to be entirely unknown to him who is deſirous of opening it. In this, according to the experience and ſagacity of the operator, the difficulties will be greater or leſs, and a very ſhallow contrivance may occaſionally preſent a greater obſtacle than a much more elaborate ſtructure. But if we ſuppoſe the ſyſtem of the lock to be known, but the particular conditions of opening it to be ſecret, the examiner will then take for his guide the probable circumſtance that the re-action of the parts may feel conſiderably different, when they are duly placed for opening, than when their ſituation is ſuch as to prevent that effect. By this clue, and by careful examination, moſt of theſe locks may be opened; and it is remarkable, that the better the workmanſhip the more eaſy it is in general to make the intended diſcovery.

Conditions of a perfect lock enumerated.

The following are the conditions which appear to me to be neceſſary in a lock of the moſt perfect kind:

1. That certain parts of the lock ſhould be variable in poſition through a great number of combinations, one only of which ſhall allow the lock to be opened or ſhut.

2. That this laſt mentioned combination ſhould be variable at the pleaſure of the poſſeſſor.

3. That it ſhall not be poſſible, after the lock is cloſed and the combination diſturbed, for any one, not even the maker of the lock, to diſcover by any examination what may be the proper ſituations of the parts required to open the lock.

4. That trials of this nature ſhall not be capable of injuring the work.

5. That it ſhall require no key;

6. And be as eaſily opened in the dark as in the light.

Theſe conditions are in ſome reſpects liable to the inconveniences already mentioned. I would therefore add the following conditions:

7. That the opening and ſhutting ſhould be done by a proceſs as ſimple as that of a common lock.

8. That it ſhould open without a key, or with one, at pleaſure.

9. That the key-hole be concealed, defended, or inacceſſible.

10. That the key may be uſed by a ſtranger without his knowing or being able to diſcover the adopted combination.

11. That the key be capable of adjuſtment to all the variations of the lock, and yet be ſimple.

12. That the lock ſhould not be liable to be taken off and examined, whether the receptacle be open or ſhut, except by one who knows the adopted combination.

Deſcription of a new lock of combination.

In meditating upon this mechanical problem, I have thought of various conſtructions, but have not yet matured one in which all the above conditions are complied with. The lock delineated in *Plate III.* poſſeſſes the firſt ſix requiſites. *Fig.* 1. repreſents the plate of the lock, of which the other ſide is ſeen at *Fig.* 4. In this laſt figure the middle piece is a handle or knob, repreſented *Fig.* 6, which, when turned, ſerves to ſhoot the double bolt *k*, *Fig.* 1, by any common connection. In the actual lock this bolt is carried backward and forward by a pin ſtanding out of *Fig.* 2, ſoon to be deſcribed. The other four circles in *Fig.* 4, are handles, repreſented in *Fig.* 5, which ſerve

Deſcription of a new lock of combination.

ſerve to move the four wheels ſeen in *Fig.* 1. Theſe wheels have twelve teeth each, and are faſtened by center-ſcrews, each upon a flat wheel of the ſame tooth; but having only ten notches actually cut, as is ſeen in the right hand upper corner, where one of the upper wheels is taken off, and is ſhewn at *Fig.* 3. Theſe upper wheels have their toothed part conſiderably higher than the interior or flat part; ſo that they would be contrate wheels if the teeth were cut quite through. But this is not the caſe, except with two of the notches, as may be ſeen in the two lower wheels more particularly, and alſo in the others. The upper wheels have alſo two of the notches between the teeth ſtopped up, as is ſhewn in *Fig.* 3; by which contrivance there are but ten ſituations for ſcrewing each wheel upon its correſpondent under wheel; and theſe ſituations are rendered preciſe, and all relative motion between the two correſpondent wheels prevented by a ſmall ſtud ſeen in the uncovered wheel, *Fig.* 1, which fits into one of the notches of the upper wheel when put in its place. The upper wheel has a number on each tooth from 1 to 9 and 0, which are of uſe for placing this ſtud. The four under wheels are held in their ſituations by four ſpring-catches, which allow them to be turned, in one direction only, by means of their knobs or handles; and when any wheel is thus turned round, the finger and thumb will feel the ſtroke of the lever, as it ſucceſſively falls into each notch, until the lever comes to reſt upon the ſmooth part. This very palpable indication then ſhews when to begin to count, calling the firſt hold or ſtroke of the catch 1; the ſecond 2; the third 3, &c.; and the lock is ſo conſtructed, that when the top wheel of any of the four couple is put on with any number oppoſite the ſtud, the ſame number counted by the catch will place the upper wheel in ſuch a ſituation, as that its notches, which paſs clear through, will lie in a circle deſcribed from the center or axis upon which the great handle turns. And therefore, when each of theſe wheels is put in its place, and the numbers known (and regiſtered, or put in the memory by ſome artificial aſſociation, ſuch as of the date of the year taken either backwards or forwards, &c.) it is only needful to move each of the four knobs till its catch has paſſed the ſmooth part, with a number of ſtrokes anſwering to its adjuſtment, and the circle indicated by broken ſhaded lines in *Fig.* 1, will be capable of paſſing through

Deſcription of a new lock of combination.

through the open ſpaces of every one of the wheels. *Fig.* 2. repreſents a contrate wheel, having its irregular portions A, B, C, D, &c. ſtanding up above its plane. Theſe portions are parts of a circle equal to that denoted by the broken ſhaded parts in *Fig.* 1. The contrate wheel is to be placed in *Fig.* 1. with its face turned down; and being there ſcrewed with its center to the central handle, it ſerves to open and ſhut the bolt, which it can only do when the four wheels are in ſuch a ſituation as to allow the circular edge-parts of *Fig.* 2. to paſs clear through their notches. If any one or more of thoſe wheels be turned ſo as not to correſpond with its number, it will be impoſſible to turn the handle, becauſe every attempt to do ſo will cauſe one of the parts of *Fig.* 2. to ſtop in one of the notches of the wheels through which it cannot paſs. The method of opening the lock will therefore conſiſt in ſetting each wheel to its known number.

As the proper ſituation of each wheel is only one out of ten, it is nine to one againſt any operator upon this lock, that he ſhall not ſet the firſt wheel right, ſuppoſing all the others in their due poſitions; but it is true that he may try all round, and will come to the right place at laſt. If two only of the wheels were deranged, it would be eighty-one to one that he ſhould not ſet them both right; and he would be deprived of any trial round a ſingle wheel, becauſe the other wheel would always hold againſt him, and prevent his knowing when the open notch of the wheel under trial preſented itſelf. Three wheels deranged would make the odds 729 to one, and the four would make the odds 6561. In the plate the combinations are ſaid to be ten thouſand, from an overſight in taking the ratio of ten to one inſtead of nine to one. But this is a matter of no conſequence as to the principles of the lock, becauſe the number of teeth or number of wheels are capable of variation. If a fifth wheel were added to this lock, the odds would amount to 59049.

As the quantity cut from *Fig.* 2. is not more than was neceſſary for the clear rotation of the wheels when the lock is ſhut, this piece, when in every other poſition, prevents the other wheels from being turned at all.

XIII.

Letter from Mr. ALEX. CROMBIE, *concerning the Caledonian Literary Society at Aberdeen.*

To Mr. NICHOLSON.

SIR,

THE want of ſocieties for ſcientific and literary improvement, has been long felt in many conſiderable towns in Scotland, and I believe in none more than in Aberdeen.

The utility of ſuch inſtitutions being ſo generally acknowledged, it is truly a matter of ſurprize to find ſo few of them in this kingdom, eſpecially when the facility of forming them is conſidered. Any attempt, however ſmall, to promote the intereſts of literature, and to diffuſe moral, political, or philoſophical knowledge among men of all ranks, will ever meet with the marked approbation of the ſincere wellwiſher to his country; and I am perſuaded you will receive peculiar ſatisfaction in being able to communicate to the public the feebleſt efforts which may be at any time directed to ſo important and deſirable an object.

Great utility of ſocieties for ſcientific and literary improvement.

In your Journal for December laſt, a *traveller* has expreſſed his ſurprize to find no antiquarian or literary ſociety, or ſubſcription library, at Aberdeen; and I agree with his remark, that thoſe who know the reſpectability of the place, cannot fail to be aſtoniſhed at it. To account for ſo ſingular a fact would perhaps be deemed preſumptuous. I have too much reſpect for my fellow-citizens to attribute it to a want of taſte, but I cannot help blaming thoſe amongſt us who are qualified for ſupporting ſuch inſtitutions, for their want of attention in this reſpect.

Reference to a letter in a former Journal.

The Profeſſors of both Univerſities certainly unite talents with influence and reſpectability,—It were to be wiſhed that they and other literary characters in town, had more concern for the improvement of the community at large, and would make ſuitable efforts to promote it.

It would be doing injuſtice to the liberality of the proprietors of the Athenæum and circulating library, to deny theſe inſtitutions their reſpective merits and advantages. But I apprehend that neither of them is ſufficient to ſupply the *de-*

The Athenæum and circulating library.

ſideratum mentioned by your correſpondent. The firſt is principally calculated for the commercial part of the inhabitants, and thoſe who have time to *lounge;* the ſecond, although compriſing much uſeful reading, is ſometimes defective in the ſelection of the books, and affords little opportunity for the union of literary exertions.

Conſideration in favour of a proprietory aſſociation.

A ſociety whoſe books are the property of the individual ſubſcribers, is far better adapted, not only for advancing knowledge and bringing uſeful talents into notice, but alſo for giving a favourable bias to the purſuits of ingenious young men of all deſcriptions, to whom ſuch a ſociety is at all times acceſſible, from the ſmall expence attending it. People become more ſolidly concerned in promoting the ſucceſs of any ſcheme, in proportion as their perſonal intereſts are interwoven with it; and we may therefore conclude, that a man will take more pleaſure, and perhaps derive more profit, from reading a book which he conſiders as his own property, than one only lent him for a time.

Subſcription-library eſtabliſhed Feb. 1805,

Impreſſed with theſe conſiderations, a few perſons in Aberdeen inſtituted a ſubſcription-library upon the 22d February, 1805, under the title of the *Caledonian Literary Society.* Beſides embracing all the periodical publications of merit in Great Britain, our ſtock is enriched with a ſelection of the moſt approved books, either preſented by the members or purchaſed from the Society's funds: Which Society has *already* increaſed to upwards of 100 members, and the liſt is daily augmenting in number and reſpectability.

at a very moderate expence.

It is worthy of remark, that the trifling ſum of ſix ſhillings *per annum* is only required from each ſubſcriber to *The Caledonian Literary Society.* So inconſiderable an expence, contraſted with the great variety of uſeful and entertaining knowledge to be derived from it, muſt form a very powerful recommendation in its favour.

We have been informed with pleaſure, that many perſons in Glaſgow, who are not members of the Society eſtabliſhed there, have contributed liberally to its ſupport by giving books—an example worthy of the imitation of others.

A Philoſophical Society in contemplation.

It is alſo in contemplation to inſtitute a Philoſophical Society, on a plan ſimilar to thoſe of London, Edinburgh, &c. for the purpoſe of receiving occaſional diſſertations on a variety of literary and other ſubjects, to be depoſited as the property, or, entered into the books of the ſociety; and afterwards publiſhed in ſuch manner as the ſociety may direct.

Should any of the friends of ſcience in Inverneſs, Banff, Peterhead, or other places, be deſirous of eſtabliſhing ſimilar inſtitutions, we will moſt chearfully furniſh them with a copy of our plan and regulations.

We have a ſincere wiſh to ſee every encouragement given to undertakings ſo laudable and beneficial, and have with this view made the preſent communication, to give publicity to ours through the medium of your excellent Journal. The inſertion of the above will oblige, Sir,

With reſpect,

Your humble ſervant,

ALEX. CROMBIE, *Pres.*

Aberdeen, January 2, 1806.

XIV.

Letter from Mr. JAMES STODART, *in Anſwer to a Queſtion concerning the Effect of the Nitrous Oxide, propoſed by Dr. Beddoes.*

To Mr. NICHOLSON.

DEAR SIR,

DR. Beddoes, in a paper on the medical effect of reſpiring the nitrous oxide, publiſhed in the laſt number of your Journal, refers to an account I formerly gave of ſome unpleaſant and rather alarming ſenſations experienced after inhaling that gas. He attributes the whole to hyſteria or nervous affection; at the ſame time ſignifying a wiſh that I would ſtate whether or not that was really the caſe. In anſwer to this I have only to obſerve, that if any ſuch prediſpoſition to hyſteria did exiſt, it was wholly unknown to me. My general ſtate of health was as uſual; nor had any thing occurred particularly to affect the mind. I had often inhaled the nitrous oxide under circumſtances in every reſpect ſimilar (at leaſt as far as I can judge) and till that time, ſo far from experiencing any thing like

Qu. Whether Mr. Stodart was nervouſly affected previous to his feeling inconvenience from nitrous oxide.

Reply: that he was not.

like debility, the very contrary effect was produced; namely, ſound and undiſturbed ſleep in the night, followed by ſtrength and increaſed chearfulneſs on the following morning.

Expectation that the nitrous oxide may prove eminently uſeful, &c.

I very ſincerely hope the medical application of this extraordinary agent, directed as it is by the very able hand of Dr. Beddoes, may prove as important and uſeful in medicine as it is intereſting and curious in philoſophy.

I have not yet heard of its being tried in caſes of ſuſpended animation; it appears to be an experiment well worth making. The ſubject is perhaps worthy of the attention of the Humane Society. I am with reſpect,

Dear Sir,

Your's ſincerely,

JAMES STODART.

Strand, January 22, 1806

XV.

Deſcription of a Statical Lamp, which maintains a Supply of Oil to the Burner from a Reſervoir, placed ſo low as to occaſion no Interception of Light. By A. F.

To Mr. NICHOLSON.

SIR,

Deſcription of a new ſtatical lamp.

I SEND you a ſketch of an overflowing lamp, of which the conſtruction will be eaſily deduced from the figure. Its advantages are, that the flame is ſupplied from below, and the light is not intercepted, but falls on all ſurrounding objects as directly as that of a candle. The upper part of A (ſee Plate IV.) contains the uſual apparatus of a lamp, either according to Argand's conſtruction or any other; and the column or tube which ſupplies the oil may be no longer than that ſupply and the conditions of the ſtructure may demand. The vaſe below contains the oil, which is poured in, when needful, at the top of the column, by a funnel or otherwiſe. The circle round B, C, repreſents a globular (or cylindrical) veſſel, having no communication with the vaſe except through a neck

Deſcription of a new ſtatical lamp.

a neck or pipe D, proceeding downwards nearly to its bottom; but there is a communication with the external air, through a perforation (repreſented by a ſmall ſhaded circle near B) which prevents the atmoſphere from interrupting the intended action. The lightly ſhaded ſemicircle B repreſents an hemiſpherical ſolid capable of revolving on an horizontal axis, ſo as to hang downwards and fill the lower half of the globe, when no fluid is preſent; or it can be raiſed up by floatage into any other poſition, according to the quantity and denſity of any fluid that may be poured in.

Let us now ſuppoſe the veſſel C to contain any fluid not more than half its capacity, and that the revolving piece B is of ſuch a weight as to be of half the ſpecific gravity of that fluid: it may then be eaſily underſtood that the piece B will ſettle into ſuch a ſituation as that part of it ſhall be immerſed in the fluid and ſupport it in the veſſel, exactly to the height of its axis. For the part of the ſolid, immerſed on one ſide, is exactly equal to the ſpace above the fluid in that ſituation, on the other ſide; and the greater part of B which is on one ſide of the perpendicular will exceed the ſmaller part on the other ſide, by exactly double that quantity. Conſequently the immerſed part of the ſolid will be preſſed down by twice its own weight; and this is exactly equal to the weight of fluid which it diſplaces; whence the body and the fluid will be in equilibrio. Let us now ſuppoſe the fluid to be brine, at the ſpecific gravity of 12, which may be poured in either at the top or at the ſide hole, and that oil of the ſpecific gravity of 9 be then poured upon it; and it is manifeſt that the oil will preſs the denſe fluid upwards into C, as repreſented in the figure, and that when C is half filled, the oil will ſtand at an elevation above the axis equal to one half more than the height of the denſe fluid, meaſured from its ſurface where the oil preſſes upon it. And, when this adjuſtment is once made, by putting in the proper quantity of denſe fluid, if any of the oil be taken out, or conſumed by burning, the preſſure will be leſs, and the denſe fluid will riſe within the vaſe. But this riſe will not be attended with any depreſſion in the veſſel C, becauſe the level will be kept up by the revolving piece B, and conſequently the oil itſelf will be prevented from falling as much as it would have done if this contrivance had not been applied.

Deſcription of a new ſtatical lamp.

I do not diſguiſe the conſideration, that as the oil diminiſhes, the diſtances between the upper and lower ſurfaces of the denſe fluid muſt diminiſh, and a proportional difference or ſubſidence in the ſurface of the oil muſt take place. The proper remedy for this appears to be that the lower ſurface ſhould be made as large as convenience will allow; that its riſe and fall may be leſs.

With regard to the diſpoſition and form of the ſpaces which are to contain the oil, it is only needful to obſerve that they may all be made ſmall or narrow, except that which is alternately to be occupied by the oil, and the denſe fluid. If the height of the denſe fluid be 12 inches, the lamp may ſtand 18 or 20 inches high, uſing ſalt water as above mentioned.

There are various practical objections to mercury; but if this fluid were to be uſed, the oil might be raiſed ten times as high, or the apparatus, if required, might be conſtructed with a leſs diſtance between the ſurfaces.*

I am, Sir,

Your conſtant Reader,

A. F.

XVI.

Letter from a Correſpondent rectifying ſome Particulars of Miſinformation reſpecting the Fiſhery of the North of Scotland.

To Mr. NICHOLSON.

SIR,

WHEN any important information is communicated to the public, we have a right to expect that it ſhould be given with extreme accuracy; or at leaſt where any doubts exiſt, with ſuch a degree of diffidence and modeſty, as may leave room for avoiding miſrepreſentation or falſhood.

I wiſh

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* The contrivance for keeping a fluid at its level by a ſemi-circular revolving ſolid was invented by Robert Hooke. See Birch's Hiſtory of the Royal Society. A. F. has ingeniouſly adopted it to a lamp which caſts no ſhadow. Hooke's lamp is nearly as faulty as the common fountain lamp in this reſpect. N.

Erroneous information reſpecting the fiſheries on the north of Scotland.

I wiſh *an Enquirer*, in your Journal for December laſt, had attended to this, before making what I conceive to be a haſty, ill-founded ſtatement, reſpecting *certain inſtances of waſteful negligence in ſome of our fiſheries in the north of Scotland*, which it is my duty at preſent to controvert.—He ſtates :

1ſt, " That the fiſhermen of Aberdeen, Banff, Peterhead, &c. never think of carrying their fiſh along the coaſt ſouthward, which they might do to Leith in twenty-four hours; or with a good briſk wind to Berwick-upon-Tweed, or even Newcaſtle-upon-Tyne; but when their reſpective towns are ſupplied, *they throw the remainder upon the dunghill for manure!!*"

From various cauſes this ſource of wealth is neglected.

A fact ſo improbable as the above, would indeed, require no ordinary ſhare of proof to gain credit to it, and I have the ſatiſfaction to aſſure you that it is entirely without foundation. The truth is, the number of hands employed in the fiſheries in the north of Scotland are ſo few, and the encouragement given to enterprize and ſpeculation in this important ſource of national wealth ſo ſmall, that no more fiſh is caught than what ſupplies the neighbouring towns. But even admitting that more were caught, and that we could vend at Leith, Berwick-upon-Tweed, or Newcaſtle-upon-Tyne, is it not to be ſuppoſed that fiſhers of places neareſt to theſe towns could afford to greatly underſell us?

The dog-fiſh caught for food, for oil, for its ſkin, and for manure.

When the dog-fiſh (*ſqualus catulus*, L.) appear on the coaſt, our fiſhers catch a great number of them and dry them for their own private uſe (for none but themſelves and the lower claſſes of people would uſe them) and likewiſe for the benefit of the oil, which they yield in great abundance, and the ſkin, which is uſed for ſmoothing the ſurface of wood. After they are drained of the oil which they contain, beſides keeping a ſufficient number for uſe, they throw the remainder on their dunghills, which produces a *valuable* manure. And no doubt your correſpondent may have miſtaken theſe for any other kind of fiſh.

He next obſerves; " That at Arbroath, another cuſtom, equally as extravagant in its kind prevails, and of which I have been a witneſs; the crab fiſhery is ſo productive, that after boiling them, the bodies of the crabs are thrown away, and the *large claws only* brought to table."

It is

The claws of crabs only are ſold at Arbroath, but the bodies are not thrown away.

It is indeed, generally the caſe here, and in every other fiſhing town, that the fiſhers for the moſt part retain the bodies of the crabs, and only diſpoſe of the claws in the public markets: but that the former are thrown away, is by no means true in almoſt any inſtance; for the fiſhermen find them of far more value in baiting their hooks, than what they could get for them otherwiſe. Indeed, if it were not for this purpoſe, it is believed, few or no crabs would be caught at all.

Much profit might be derived by a company if eſtabliſhed at Aberdeen for exporting white fiſh.

Having thus endeavoured to vindicate our fiſhers from the charge of waſteful negligence, which none who know them will think them guilty of; I cannot conclude without expreſſing my ſurprize that no company has yet been eſtabliſhed at Aberdeen for exporting white-fiſh. It is obvious from its excellent ſituation, and advantages, that very handſome profits could be cleared, if ſuch an undertaking were once ſet on foot, and well conducted; equal, if not ſuperior to the ſalmon fiſhing, which it is well known has been greatly the means of enriching this place.

If you deem the above obſervations worthy a place in your uſeful Journal you will oblige,

SIR,

Yours reſpectfully,

A. L.

Aberdeen, January 3, 1806.

XVII.

Obſervations and Enquiries concerning the Heat of Air blown from Bellows. By K. H. D.

To Mr. NICHOLSON,

SIR,

Paſſage from Dr. Black's lectures.

I BEG leave to mention a paſſage in Dr. Black's Lectures on the Elements of Chemiſtry, publiſhed by Profeſſor Robiſon, which occurs at page 88, Vol. I.

The author is ſpeaking of the communication of heat, and has, in the former part of the page accounted for the apparent coldneſs of a ſtream of air, by its preventing the accumulation of heat around our bodies, by its impulſe and rapid ſucceſſion, both cooling our clothes faſter, and carrying away the warm air that was intangled in them. The Doctor ſays, "the ſenſation of coldneſs, therefore, produced by wind, or agitated air, is ſo much ſtronger than that produced by equally cold air in a ſtagnating ſtate, that we are often perſuaded the agitated air is actually colder, until we examine it by the thermometer; and Dr. Boerhaave thought the deception ſo ſtrong, that he contrived an experiment to remove it completely (Boerhaave Elementa Chemiæ.) He ſuſpended a thermometer in the air of a large room for ſome time, and noting the degree to which it pointed, he then directed againſt the bulb of it a ſtream of air impelled by a large bellows in the ſame room;—that ſtream of air would certainly feel to a perſon who oppoſed any part of his body to it, conſiderably colder than the reſt of the air in the ſame room; *but the thermometer is not in the leaſt affected by it.* And it would be eaſy to exhibit another experiment to ſhew, that agitated air is not made colder by agitation. A piece of ice, for example, being ſuſpended in the air of a warm room, and blown upon by bellows, inſtead of being thereby kept the more cool, as our hand would be, and preſerved the longer from being totally melted, would certainly be melted ſo much the faſter, than when the air is allowed to ſtagnate in ſome meaſure around it."

that agitation of the air, though it cools heated bodies, does not render the air colder, nor hotter, though it melts ice.

I take the liberty of troubling you with this in conſequence of a communication from your ingenious correſpondent, Mr. Richard Winter, publiſhed in the laſt Number of your excellent Journal, where his experiment on the effect produced on a thermometer by a blaſt of air from a pair of bellows, directly contradicts Dr. Black's aſſertion, that "the thermometer is not in the leaſt affected by it."

M. Winter found the air from bellows gave out heat.

That there is great truth in Dr. Black's general ſtatement of the fact, of a blaſt of air cooling a body warmer than itſelf, by affording a continued ſeries of freſh ſurfaces to carry off the caloric, I have no doubt, and that it ſhould have an equal effect in warming a body colder than itſelf, ſeems equally evident, or by ſupplying the colder body with caloric. But in the caſe of the thermometer being raiſed four degrees, (as ſtated in Mr. Winter's experiments) we are not told that it was of a temperature lower than that of the air of the room

Queſtions reſpecting theſe facts.

room. How then, Sir, are we to reconcile the reſult of your correſpondent's experiment with Dr. Black's aſſertion, mentioned above?—Are we to ſuppoſe the blaſt of air to have actually acquired an increaſe of temperature, and if ſo, how has it acquired it? I hope your correſpondent (ſhould this ever reach his ears) will not imagine I doubt the accuracy of his experiment; my only object is, the clearing up a circumſtance, which at preſent is to me at leaſt, not by any means ſatisfactorily accounted for. To whom then can I better apply, than to you, if indeed I may venture to hope you may think the object worthy of your conſideration? Whether that ſhall prove the caſe or not, I muſt always feel (in common with thouſands of others) the benefit you confer on the ſcientific world, by the eaſy means of communication of knowledge to the public, which your Journal affords,

I have the honour to be,

Sir,

Your obedient Servant,

K. H. D.

Tunbridge,
January 19, 1806.

P. S. I do not underſtand how the ſuppoſed greater capacity of a vacuum for caloric explains the facts, whether of the riſe of the mercury in the thermometer, or the melting of the ice.

Obſervations on the preceding Letter, by W. N.

It is deſirable that the experiments ſhould be repeated.

WHEN a queſtion ariſes concerning the diſagreement of facts, the proceſs obviouſly indicated is to repeat the experiments; in order that it may be ſeen what circumſtances may have tended to produce miſtake, or what may have been the real difference between operations ſuppoſed to be the ſame. Though I have not had an opportunity of doing this, I have nevertheleſs thought it proper to make a few remarks.

Agitation enables a fluid to gain the common temperature more ſpeedily.

When a body is immerſed in the air, or in any other fluid differing from itſelf in temperature, the body will acquire the common temperature more ſpeedily (that is to ſay, it will be heated or cooled more quickly) by agitating the fluid, than if it were left undiſturbed;—and this for the plain reaſon, that more of the particles at the original temperature will come into

Air is heated by condenſation.

Whence the rear blaſt of bellows will be hot, and the remoter will act only by its motion.

int contact with it in the latter than in the former caſe. Theſe remarks ſupport and explain the facts noticed by Dr. Black and Boerhaave. Agitation of the air is merely ſuppoſed, and not that it ſhall be either condenſed or rarified. Many facts concur to ſhew, that the capacities of elaſtic fluids for heat are encreaſed by rarefaction, and diminiſhed by condenſation; proofs of which we have by experiments in the air-pump and condenſer, and in the late experiments of exploſions produced in the chamber of the condenſing ſyringe. If we attend to this law, we muſt infer that the air in a pair of bellows, being ſuddenly compreſſed by a force perhaps equal to one twentieth of an atmoſphere or more, will acquire an increaſe of temperature; and if in this diſpoſition to give out heat, it be made to ruſh againſt the ball of a thermometer, it will heat the mercury, and cauſe it to riſe in the tube. Now, in order to reconcile both the reſults of Mr. Winter, and of Boerhaave to truth, we muſt recollect that bellows, like the unfortunate traveller in Eſop's Fables, can blow hot and cold at the ſame time. If the thermometer be held very near the aperture, the warm air will heat the mercury; but if it be held at a greater diſtance, where the warm air has become plentifully mixed with cold, the effect of its temperature may be altogether inconſiderable, while that of the agitation continues to be effective: that is to ſay, the thermometer if already at the common temperature, will neither riſe nor fall; if it be already hot the ſteam will cool it; or if cool the ſteam will heat it. Thus it is, to return to our traveller, that we breathe upon our fingers held cloſe to our mouth when we mean to warm them; but when we wiſh to produce cold, we hold the ſubject at a diſtance, and blow at it.

As the thermometer falls in the pneumatic vacuum, I ſuppoſe there may be ſome miſtake in the poſtſcript.

XVIII.

Account of the Performance of the patent Ship Economy at Sea, in a Voyage to the Weſt India Iſlands, and of ſome Improvement in the Tackle aboard, proved of great Utility. By Mr. J. Whitley Boswell.

To Mr. NICHOLSON.

Dear Sir,

Deſcription of the ſhip's conſtruction has been publiſhed in a former number of this work.

AS in a former number of your Journal *, you favoured me by inſerting a deſcription of the conſtruction of the ſhip Economy, built according to my patent, I hope you will alſo admit the following account of her performance at ſea, and of ſome other matters; of conſiderable utility to naval concerns.

The ſubject proper for the Journal as containing an account of an experiment in the arts important to the nation, and on a great ſcale.

Your Journal is principally devoted to the furtherance of the moſt uſeful of all knowledge, that of experiments in Philoſophy and the Arts. And to a nation which like this depends on its ſhipping for moſt of the many advantages it enjoys over the reſt of the world, what experiments can be more important, or ought to be more intereſting, than thoſe which concern this ſubject?

Gentlemen who have aſſiſted in this experiment.

The plan muſt be of great uſe to the nation when adopted,

The experiment which has been made on this occaſion is entitled to a farther ſuperiority over other uſual experiments, an account of the large ſum of money required for conducting it, which altogether rather exceeded 5000*l.* and on this occaſion it is but juſtice to mention the ſpirit with which Wm. Luſhington, Eſq. of this City, and Richard Griffith, Eſq. of Dublin have came forward to aſſiſt in making this experiment, whoſe property the ſhip principally is, (my ſhare of it being comparatively ſmall to theirs); to thoſe gentlemen this country is chiefly indebted for proving a matter of great utility to its naval concerns, and which ſooner or later muſt be of the greateſt advantage to it, when the plan comes into uſe, though the ſpirit of the times may defer this period until it ſhall ceaſe to be of any benefit to us, and others may reap the profit of theſe gentlemens public ſpirit and my labour and ſtudy; but as I waited till I ſhould have the proof of actual experiment to add, to that of a theory (which though founded on unerring principles, and of which each part had been often proved in detail before, it could not be expected to convince thoſe whoſe preſſure of buſineſs, or want of taſte for ſuch ſtudies, deprived of time, or inclination, or made it too great a labour to attend to its demonſtration in any other way) I ſhall hope now, (that my exertions to bring this plan of ſhip-building into the notice it deſerves, when its ſufficiency, ſtrength and ſecurity is,) ſupported by actual and ſevere proof, will meet with a fair and candid conſideration, from the direction of our navy, and thoſe whoſe commercial purſuits lead to employ veſſels of great burden.

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* Vol. IX. p. 166.

hopes that the ſucceſs of the experiment will accelerate this period.

The chief advantage of this method of ſhip-building is, that it enables the builder to uſe timber of much leſs coſt, and vaſtly more eaſy to procure, with ſtrength and ſtability ſuperior to the old method, in proportion to the quantity of timber, and to diſpenſe with knee timber entirely.

Economical advantages of this method of ſhip-building, cheaper timber, more eaſily procured, may be uſed in it.

In a national point of view this method is ſtill of greater benefit; for as it admits of timber of fifty years growth to ſupply the place of that of one hundred, not only the foreſt lands may be made to produce timber for double the number of ſhips for our navy in a given time, but private gentlemen would be alſo induced to plant more timber for this purpoſe, from the ſuperior profit they could in this caſe make of their plantations, and the hope it would give them of being able to receive the fruits of their labour during their own lives, which at preſent can only be expected to be reaped by their grand children.

An oak of fifty years growth has alſo a much greater quantity of ſerviceable timber in it, in proportion to its age, than one of an hundred years, and four times the number of them at leaſt can ſtand and flouriſh at one time at the ſame extent of ground; ſo that the public would be benefited by the adoption of the plan every way; for while timber would thus be rendered more plenty, thoſe who prepared it for market would alſo obtain a greater profit.

Its national advantages, the foreſts could ſupply double the quantity of the timber wanted in this plan in a given time, more timber would be planted if it was adopted; oaks of fifty years, have more ſerviceable timber in proportion than thoſe of 100, and occupy only one fourth the ground.

Hitherto the price of timber for the navy has been attempted to be kept down by arbitrary regulations, which tended to encreaſe its ſcarcity; at laſt, notwithſtanding every effort, the price and ſcarcity have encreaſed ſo much that our government have been forced to the expedient of partly relying on a foreign country for the continuation of the navy; and to

The ſcarcity of ſhip timber has compelled this country to build ſhips of war in Ruſſia; danger of this expedient.

depend on the dock yards of Ruſſia for the bulwark of the Britiſh nation, for the defence of its liberties, and of its political exiſtence, and this at a time when our crafty and implacable foe has got poſſeſſion of nearly all the foreſts of the reſt of Europe, and is making the moſt prodigious exertions to out-number our navy.

Should induce the trial of the patent plan, in doing which there is no riſk, as its ſufficiency has been proved.

If my plan of ſhip-building tends in ſo great a degree to diminiſh thoſe difficulties, and even dangers, as is ſtated above, is it not worthy of a trial at leaſt, even if ſome riſk was run in that trial? but when no riſk is run, when the plan has been proved, the moſt ſcrupulous economiſt of the public wealth can ſtart no objection to that trial of it in the navy, that the public neceſſity for ſome expedient to ſupply timber for its uſe ſo loudly calls for.

No public money required to make experiments, they have been already made at the expence of the owners of the ſhip.

We aſk no drafts on the public ſource to try experiments on the ſubject, theſe have been already compleatly made at our own expence, and all we demand is our country to condeſcend to reap the fruit of our exertions; if ſhe does, we ſhall rely on her generoſity to recompence us, convinced that ſhe will have ample proof that we have deſerved it; but ſhould this not be the caſe, we will not reſt contented with having diſcharged our duty, in doing the moſt we could to ſerve her; which if we ſhould be ſo happy as to effect, we will never regret our trouble or coſt.

The performance of the ſhip at ſea cannot be miſtated on account of its publicity.

Having thus ſtated the claims which the ſubject has to public attention, I ſhall proceed to relate the performance of the ſhip at ſea, which, as ſhe ſailed in company with a large convoy both out and home, is a matter of too public a nature to admit any miſtatement I might wiſh to make, which God knows is far from my deſire.

On the 22d of Auguſt, 1804, The patent ſhip Economy weighed anchor off Graveſend, with but a ſmall cargo aboard, as is uſual for ſhips outward bound to her deſtination, and ſet ſail on her voyage to Trinidad and Grenada; and on the 14th October following arrived at Grenada; her performance on this voyage is beſt ſtated in her Captain's own words, in the following extract from a letter to Wm. Luſhington, Eſq. London.

SIR, *Grenada, Oct. 15th, 1804.*

The Captain's letter relative to the voyage out.

I have the pleaſure to inform you of the ſhip Economy's ſafe arrival here yeſterday evening. We had a fine paſſage, and had but one gale of wind: The ſhip performs as well as it is poſſible for a ſhip; is remarkable eaſy at ſea, ſteers and ſails well, and is perfectly tight. In the gale of wind the Epervier man of war ſprung her foremaſt; the Robert Aylward ditto; a brig, Maſter or brig named Swinger, loſt both topmaſts and parted convoy in lat. 14. 30 N. Our ſhip behaved extremely well and never ſtrained a rope yarn.

Ship ſteers and ſails well, is remarkably eaſy at ſea, perfectly tight in a heavy gale, in which other ſhips ſuffer much, ſhe meets no accident;

(*Signed*) ALEXANDER SMITH.

From the period of this letter ſhe remained at the Weſt India iſlands until the 23d of July 1805; being detained there the greateſt part of that time by the arrival of the French fleet, which was afterwards chaſed back to Europe by the gallant and ever to be regretted Lord Nelſon; from the 23d. of July, when ſhe ſailed for England, to the 29th of Oct. when ſhe caſt anchor off Portſmouth on the Mother-bank, ſhe experienced a ſeries of ſevere weather and violent gales of wind, in which ſome of the fleet with which ſhe returned foundered, and others were obliged to bear away for America for ſhelter. The remarkable bad paſſage home of the Leeward iſland fleet, of which ſhe was one, is too well known to need much deſcription: all ſeamen muſt be ſenſible that three months toſſing on the Atlantic ocean in ſuch hard weather, beating up againſt contrary winds, to a veſſel as deeply laden with ſugar as the ſkrews could compreſs it into her, muſt have been a moſt ſevere trial, and that if ſhe had a ſingle weak part, or defective principle in her conſtruction, it muſt have given out in that time: but while moſt of the other ſhips of the fleet met with more of leſs damage both to themſelves and their cargoes, ſhe bore through all without the ſmalleſt accident, and brought home her ſugar perfectly dry and ſafe; which was not compleatly diſcharged until Jan. 1806 (on account of her detention at Portſmouth, through contrary winds from whence ſhe ſhe did not get to London before the 27th. of Nov. on which day ſhe hauled into the Weſt India dock,) or this account would have been made public before. A further proof of the ſtability of her frame work, is her taking the ground with a full cargo on board without any accident, as may be ſeen more particularly in the following account of her performance home which I received from her Captain.

delayed at the Weſt Indies by the French fleet,

returns home, experiences violent gales, and a tedious paſſage of three months, ſome ſhips of the fleet founder through ſeverity of weather.

Why this ſevere trial is a ſufficient proof of her ſtability, when deeply laden,

took the ground without injury at Trinadad.

DEAR SIR, *January* 17, 1806.

"IT is with pleaſure that I have leiſure to inform you of the performance of the patent ſhip Economy, during the voyage under my command.

Captains account of the voyage home, In a violent gale the ſhip performs extremely well, and is a good ſea boat: two ſhips founder in this ſtorm, one abandoned, another large ſhip rendered unmanageable, and taken in tow, and ſeveral others much damaged.

"On the fifth and ſixth of September laſt, latitude 37°, 34 N. we experienced a very heavy gale of wind, with an heavy croſs ſea, occaſioned by the wind ſhifting to different points of the compaſs ſuddenly, and blowing with extreme violence: during the whole of the gale, the Economy behaved as well as I ever experienced a ſhip to do, and much better than could have been expected for ſo ſmall a ſhip; in fine, ſhe is as good a ſea boat as ever put keel in ſalt water. During the gale, two ſhips, it is ſuppoſed, foundered; after the gale one was abandoned as not tenable, ſhould another gale of wind come on: the Prince of Wales, a ſhip of 300 tons, had every thing waſhed from her deck: The Princeſs of Wales, a ſhip of the ſame ſize, broke her rudder, and was left in tow of the Hyæna ſloop of war. Several other ſhips met with conſiderable damage, which proved undeniably the violence of the wind. Notwithſtanding the lumbered ſtate of the Economy, we loſt nothing off deck, and I don't think there was a ſhip, large or ſmall in the fleet, that made better weather: ſhe did not ſail ſo faſt coming home as going out, but that is eaſily accounted for, when we conſider ſhe was not coppered, and was out fifteen months on a wooden ſheathing, with barnacles as long as your finger on her, and the bottom reſembling a rock; and was beſides laden as deep as ſhe could ſtow. She works and ſteers amazingly well. I would not wiſh to change her if ſhe had been larger, but being only 200 tons, ſhe is too ſmall both for my intereſt and the Weſt India trade.

The Economy meets no accident, and is very weatherly: tho' deep laden; has a foul bottom which impedes her failing, ſhe works and ſteers well.

She remains perfectly tight after the ſevere paſſage, though run aground at Trinadad with a full cargo of ſugar;

"The ſhip has been perfectly tight all the voyage, although we had a very tempeſtuous paſſage, and likewiſe ran her on ſhore, *ſugar loaded*, under the batteries at Trinadad, to prevent her falling into the hands of the French, as we ſuppoſed, where we lay for twenty-four hours, until we diſcovered that it was Nelſon's fleet. In my opinion ſhe is one of the ſtrongeſt ſhips in the river Thames of her ſize.

is a very remarkable ſtrong ſhip,

"The new iron ſlings and other iron work on the yards exceed my moſt ſanguine expectation, I have ſeen the ſhip covered with flaſhes of lightning when at Trinadad, and never experienced the leaſt injury from ſo much iron being about the yards, owing to the precaution which I took of ſerving the iron work and paying it with pitch, which I think ſerved as a non-conductor. I have a higher opinion of iron work than ever I had, and think the iron rigging in the plan we uſed to talk about while the ſhip was building, would anſwer to admiration, and might be the means of preſerving the maſts of men of war, when in action, as being leſs liable to be cut with ſhot. When I can manage it, I mean to rig the mizen maſt of a ſhip wholly with iron, to give it a trial. When I examine the bottom, I will give you my opinion of the pieces of ſheathing ſteeped in your preparation to prevent the worms from deſtroying the bottom. The large rollers which you had let in beneath the hawſe holes for the cables to work on, were of very great benefit, and I think ſaved us the labour of two men in weighing anchor, they alſo prevented the wear of the cables very much, and were greatly liked by the ſailors, as making the purchaſe more lively,

of the iron ſlings to her yards, and captain's high opinion of the uſe of iron work in rigging. State of experiment on part of the ſheathing will be attended to, great uſe of the large rollers for the cables.

Your's very ſincerely,

ALEX. SMITH."

The iron ſlings which Captain Smith mentions, were on a plan of his own, and different from thoſe uſed in men of war, in not requiring above three or four feet of chain for each yard, and ſerved merely to ſuſpend the yards from the point of the tops; which method greatly ſaved the wear of the maſts, and permitted the yards to work more freely. Iron ſtraps were alſo uſed to moſt of the blocks inſtead of hemp.

Further account of the iron ſlings,

The rollers for the cables were about fourteen inches long and eleven in diameter, and worked on iron gudgeons about two inches in diameter, in braſs ſockets. The rollers which have been hitherto uſed for this purpoſe, were generally much too ſmall, ſeldom exceeding the diameter of the cable; which diminiſhed ſize both increaſes the friction and injures the cable, from the ſmallneſs of the nip which they occaſion; or, in other words, from the acuteneſs of the angle at which the cable is forced to bend in paſſing over them.

and of the cable rollers,

In concluding this account I beg leave to mention, that I could, in building another ſhip, greatly diminiſh the ſpace neceſſary for the tranſverſe forms uſed in my plan, by ſetting them farther aſunder, and forming them of iron, which method

Iron tranſverſe frames may be uſed in future in this place, to ſave room.

—and the fore and aft ribs ſcarped in a more economical method.

method is ſpecified in my patent, and that I could alſo make a great ſaving in the timber uſed in the fore and aft ribs, by a method of ſcarping them, alſo within the limits of my ſpecification. Experience has ſince convinced me of the ſuperiority of both theſe methods, of which I had ſome doubt when I built the Economy, or they ſhould have been uſed in her.

It may ſeem paradoxical to aſſert that iron is oftentimes cheaper than wood in ſhip building, when it can be uſed: but a plain proof of this exiſts in the bow of the Economy, of which the three lower breaſt-hooks are iron of conſiderable ſubſtance, and yet coſt leſs individually than any of the wooden ones above them, though theſe are of no extraordinary girth, or of much curvature.

The Economy will remain a few weeks at the London Docks for inſpection.

The Economy will be a few weeks in the London Docks, where ſhe has now moved, for the inſpection of the public, and where all gentlemen who are intereſted in ſhipping concerns may ſee her conſtruction; and thoſe who examined her previous to her ſailing, may convince themſelves that I have exaggerated nothing, as to the ſound ſtate in which ſhe has returned from her tempeſtuous voyage.

Dear Sir,

Your very humble ſervant,

J. WHITLEY BOSWELL.

XIX.

Experiments on the Torpedo. By Meſſrs. HUMBOLDT *and* GAY LUSSAC. *Extracted from a Letter of M. Humboldt to M. Berthollet; dated Rome, 15 Fructidor,* Year 13 (Sept. 2, 1805.)*

THE curious theory with which Volta has enriched the ſcience of natural philoſophy, on the ſubject of electric fiſh having been received as authentic by many naturaliſts, renders the phenomenon of the Torpedo worthy of farther inveſtigation. You know, my dear friend, what was our impatience to procure theſe fiſh, and will perhaps be ſurpriſed that ſo much time ſhould elapſe without having heard from us on

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* Annales de Chimie, Vol. LVI.

The torpedo found at Genoa and Naples, but not at Civita Vecchia.

The ſhock of the torpedo feels different from that of electricity.

ſubject. At Genoa, we perceived ſome; but we were then without our inſtruments. At Civita Vecchia we ſought them in vain. But during our ſtay at Naples we frequently procured ſome very large and lively ones. In this letter you will find detailed the experiments made by M. Gay-Luſſac and myſelf on the powers of this fiſh (*Raja-torpedo* of Linneus). M. de Buch, a German mineralogiſt, well acquainted with all the branches of phyſical ſcience, was witneſs to our proceedings. I ſend you the reſults, giving ſimple facts, unmixed with theoretical ſpeculations. Our experiments were chiefly directed towards the diſcovery of that ſtate of the torpedo when it was leaſt capable of exerting its power upon the human frame. This power has been generally deſcribed as electrical; but the ſenſation produced by it is materially different from that cauſed by the diſcharge of a Leyden phial — Having no other book by us beſides the work wherein Aldini* combines the reſearches of Geoffroy with thoſe of Spallanzani and Galvani, it is not to be expected that we ſhould compare our experiments with thoſe which may have been previouſly made by other philoſophers.

Powers of the torpedo inferior to thoſe of the gymnotus of S. America. Shock of the torpedo more violent than that of electricity. It acts under the water,

1. Though the ſtrength of the torpedo is far inferior to that of the gymnotus, it is equally capable of cauſing painful ſenſations. A perſon much accuſtomed to electric ſhocks, can hardly ſuſtain that of a lively torpedo of four decimeters (16 inches) in length. The animal acts under water, and it is only when it loſes ſtrength that the fluid impedes its action. In this caſe, M. Gay Luſſac obſerved that the ſhock is not perceptible till the fiſh is raiſed above the ſurface.

—and ſeems to uſe more effort than the gymnotus.

2. I obſerved, when in South America, that the gymnotus gives the moſt violent ſhocks, without any exterior movement of the eyes, the head, or the fins: it appeared as tranquil as a perſon when paſſing from one idea to another, or from one ſenſation to another. Not ſo the torpedo: We obſerved a convulſive movement of the pectoral fins, each time it gave a ſhock, which was more or leſs violent according as the ſurface was larger or ſmaller wherein the contact took place.

Shocks from the torpedo and gymnotus cannot be obtained but by irritating the animal.

3. The powers of the torpedo and gymnotus cannot be excited at pleaſure, as we ſhould diſcharge a Leyden phial or a

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* Memoires ſur la Torpille, dans l'Eſſai ſur le Galvaniſm, Vol. II. p. 61.

conductor. A ſhock is not always felt on touching an electric fiſh; it muſt be irritated before it will give the ſhock. This action depends on the will of the animal, whoſe electric powers perhaps, are not kept conſtantly charged; yet it can recover them with wonderful celerity, as it is capable of giving a long ſucceſſion of ſhocks.

The ſhock obtained by a mere touch with the finger,

4. The ſhock is felt (the animal being diſpoſed to give it) as well on touching with one finger a ſingle ſurface of the electric organs, as on applying the two hands to the two ſurfaces, the upper and under, at once. In both caſes it is immaterial whether the perſon applying his finger or his two hands, be inſulated or not.

—but the contact muſt be direct. Metals ſeem to be non-conductors of the ſhock of the torpedo,

5. When an iſolated perſon touches the torpedo with a ſingle finger, it is indiſpenſible that the contact be immediate, as no ſhock will be felt if a conducting body (of metal for example) be interpoſed between the finger and the organ of the fiſh.—For this reaſon, the animal may be touched with impunity by means of a key, or any other inſtrument of metal.

6. M. Gay-Luſſac having made this important obſervation, we placed a torpedo on a metal diſh, with which the inferior ſurface of its organs were in contact. The hand which ſupported this diſh experienced no ſhock, whilſt another iſolated perſon irritated the animal, whoſe convulſive movement of the pectoral fins indicated a moſt violent emiſſion of the electric fluid.

Experiments which ſhew that they conduct.

7. When on the contrary, a perſon held the torpedo in a metal diſh in his left hand (as in the preceding experiment), and with his right touched the ſuperior ſurface of the electric organ, he experienced a ſmart ſhock in both arms at the ſame moment.

8. The ſame was felt, on placing the fiſh between two metal plates, whoſe edges were not in contact with each other, and applying the two hands at once above and below them.

9. But if the edges of the metal plates be ſuffered to touch each other, no ſhock will be felt in either arm. The communication between the two ſurfaces of the organs is, in this caſe, formed by the plates; and the new connection ariſing from the contact of the two hands with the plates is without effect.

The organs of the torpedo have no influence on the electrometer.

10. The moſt ſenſible electrometer manifeſted no electrical tenſion in the organs of the torpedo; in whatever way it was applied, it was not in the leaſt affected; neither, on directing

it towards the organs, nor in inſulating the fiſh, covering it with a metallic plate, and making a communication between this plate, by means of a conducting thread, and the condenſer of Volta, was there any indication (as with the gymnotus) that the animal affected the electric intenſity of ſurrounding bodies.

Examination of the conducting powers of water.

11. As electric fiſh, when healthy, exerciſe their powers as forcibly beneath the water as in the open air, we were led to examine the conducting properties of this fluid. Several perſons formed a chain of hands between the ſuperior and inferior ſurfaces of the organs of the torpedo: the ſhock was not felt until they had wetted their hands. The action was not intercepted when two perſons ſupported the torpedo with their right hands; and inſtead of holding each other's left hand, they each plunged a metallic rod into water placed upon an iſolated body.

Flame does not conduct the ſhock.

12. By ſubſtituting flame in lieu of water, the communication was deſtroyed, until the rods touched each other in the flame.

No ſhock can be obtained without immediate contact with the fiſh.

13. It muſt, however, be obſerved, that in water, as in air, the ſhock was not perceptible without an immediate contact with the body of the electric fiſh: the leaſt poſſible intervention of the water prevented it. This fact is the more remarkable, as it is known that in galvanic experiments, where the frog is immerſed in water, it is ſufficient to direct the ſilver forceps towards the muſcles to cauſe a contraction, though a body of water be interpoſed, equal to one or two millimetres in thickneſs, or about one-twentieth of an inch.

Organs of the torpedo not ſuſceptible of any exceſs of charge.

Doubt whether the ſhock of the gymnotus can be felt without actual contact with it.

Torpedo ſuppoſed to act by an electric equilibrium, the oppoſite ſtate being very near.

Theſe, my dear friend, are the principal obſervations which we have made on the torpedo. The experiments, No. 4 and 10, prove that the electric organs of theſe animals are not ſuſceptible of any intenſity or exceſs of charge. Their action may rather be compared to that of a combination of Leyden phials, than to the conductor of Volta. Without communication no ſhock could be felt: and having experienced the power of the gymnotus through very dry cords, I imagine, that where I have been affected by this powerful animal without direct contact, it had been occaſioned by ſome deficiency in my inſulated ſtate. If the torpedo act by poles, that is by an electric equilibrium which poſſeſſes a tendency to repleniſh itſelf, experiments 5 and 6 ſeem to prove that theſe poles exiſt near each other, on the ſame ſurface of the organ. The ſhock is felt on merely touching the ſurface with the finger. A plate interpoſed between the hand and the organ, (*Exp.* 6,) re-eſtabliſhes the equilibrium, and the hand which ſuſtains the plate is not affected, becauſe it is placed beyond the current. But if we ſuppoſe an heterogeneous number of poles upon each ſurface of the organ, whence does it ariſe, that, in covering theſe ſurfaces with two metal plates, whoſe edges do not touch each other, and placing the hands on theſe plates, the equilibrium ſhould be found in the arms? Why, it may be aſked, does not the poſitive electricity of the inferior ſurface ſeek at the moment of exploſion the negative electricity of the next or neareſt pole, but rather ſeek it in the ſuperior ſurface of the electric organ? Perhaps theſe difficulties may not be inſurmountable; yet the theory of theſe *vital actions* well deſerves attentive reſearch. Geoffroy has proved that thornbacks, who give no ſigns of electricity, are furniſhed with organs analogous to thoſe of the torpedo. The leaſt injury on the brain of the torpedo deſtroys its electric powers. The nerves are no doubt concerned chiefly in theſe phenomena; and the phyſiologiſt who ſhould admit the power of vital actions, might with ſucceſs oppoſe the theory of the naturaliſt, who would endeavour to explain all by the contact of the albumino-gelatinous pulp of the nervous laminæ wherewith nature has endowed the organs of the torpedo.

Objections to this notion.

Conſiderations of theory.

SCIENTIFIC NEWS.

Prizes propoſed by the Univerſity and Academy of Wilna, in June, 1805.

CLASS OF SCIENCE AND MEDICINE.

Firſt Prize.

To determine whether ſaccharine ſecretions take place in other organs beſides thoſe affected in diabetes mellitus.

BESIDES the diabetes mellitus of the authors on medicine, are there any other diſorders peculiar to man, which, according to experiments well aſcertained, produce in different organs a ſecretion ſimilar to ſugar, ſufficiently abundant to finally occaſion conſumption? And what are theſe diſorders?

In a note on this ſubject it is recommended to examine for ſaccharine matter, the fluid ſubſtance of colliquative ſweats; that produced in the *fluxus cœliacus*, and in the pituitous conſumption from lungs, which after death are not ulcerated; and the milk of women afflicted with the *gallactirhœa*.

Second Prize.

What are the true characters and the cauſes of the malady, which although not excluſively appertaining to Poland, is however called the *Plica Polonica*? Are there any means of curing this diſeaſe more ſucceſsful than thoſe hitherto employed? and what are theſe means?

To aſcertain the true cauſe and cure of the Plica Polonica.

Third Prize.

What are the principal maladies of vegetables? And what is the true analogy between them and thoſe of animals?

Relative to the diſorders of vegetables.

CLASS OF NATURAL PHILOSOPHY AND MATHEMATICS.

Prize.

Suppoſe a canal, through which a certain quantity of water *m*, flows in a given number of ſeconds, through a tranſverſe ſection of a given depth and breadth, terminated by the two banks: If on this ſection a dam is conſtructed, at the top of which an opening is made for the water to paſs, of given dimenſions; it is demanded according to what law the water, elevated by the obſtacle which the dam preſents, will be forced to riſe not only at the dam, but backwards along the canal.

Formulæ are required ſufficiently general, to be applied not only to the quantity of water *m*, but to any other $m+x$. Experience not exactly agreeing with the theory, the neceſſary corrections muſt be made to the formulæ, and proofs given from facts and obſervations, ſhewing how nearly they approach the truth.

The law is demanded, by which water riſes in a canal behind a dam, at the top of which a given opening is made.

CLASS OF MORAL AND POLITICAL SCIENCES.

Prize.

As the ſciences of natural philoſophy and mathematics make daily advances, and are enriched with new diſcoveries, it is demanded—

Qu. why moral ſciences do not make the ſame progreſs as the phyſical?—If they can be farther improved?—What are the bounds to their perfectibility?—What are the beſt methods to attain this point?

1ſt. Why the ſame does not take place in the moral ſciences?

2d. Whether among the different branches of theſe ſciences, there be any capable of a farther degree of perfection? And what theſe are?

3d. To what degree are they of this nature? And what are the limits to their farther improvement?

4th. What are the moſt proper methods to advance the moral ſciences to this boundary of perfection?

It is deſired that the diſcuſſion of this ſubject may be conducted ſo as to preſent reſults, which may contribute to the perfection of that theory of Legiſlation, which is moſt conformable to the nature of man.

Second Prize.

Tenets of Adam Smith and Dr. Queſnay?

To determine (by making an analyſis of political economy) what are the points in which the leading notions of Adam Smith and Doctor Queſnay agree, and in what they differ, or are oppoſite?

This examination muſt neceſſarily produce reſults uſeful to the progreſs of political economy.

Amount of prizes, and laſt days for receiving memoirs.

The prize for each of theſe queſtions is 100 golden ducats of Holland (46*l.* 5*s.*); and the laſt day for the reception of memoirs on medical ſubjects, the 31ſt Auguſt, 1807; and for the others, the ſame day and month in 1806.

Conditions to be obſerved by the Candidates.

To each memoir ſent in muſt be attached a ſeparate and ſealed note, containing the title of the work, and the name and addreſs of the author: This note will only be opened by the Univerſity if the work ſhall obtain the prize.

Memoirs to be written in Latin, French, or Poliſh.

The memoirs muſt be written legibly, in Latin, French, or Poliſh languages. The packet ſhould be addreſſed to the Rector of the Univerſity of Wilna, and addreſſed to one of the bankers of that city, MM. Keyſer or Karner, that it may go free. The Rector will give a receipt to theſe bankers.

The Univerſity ſhall not be obliged to return either the memoirs or the drawings ſent; but the authors will always be permitted to take copies of them.

Conditions relative to property, copy right, &c. of the memoirs.

The Univerſity engages not to print any of the works ſent them, without permiſſion of the authors; but the authors may, at any time, print them if they think proper.

The diſtribution of prizes ſhould take place before the termination of the years in which they are to be determined. The prizes adjudged ſhall be publiſhed in the gazette.

Time of diſtributing prizes.

The author ſhall receive his prize from the adminiſtrative committee of the Imperial Univerſity of Wilna, either in perſon or by deputy. The prize will be at his option, either a gold medal or 100 golden ducats of Holland.

The Profeſſors and honorary members of the Univerſity of Wilna, cannot be candidates for the prizes.

Profeſſors of Wilna not to be candidates.

Revived Precipitates from alkaline Solutions of metallic Oxides.

M. Klaproth, a little before his deceaſe, diſcovered that the ſolution of the metallic oxides in the alkalis, are as eaſily precipitated in their metallic ſtate, by the other metals ſoluble in the ſame alkalis, as are the acid ſolutions of theſe metals by phoſphorus: He has made a very ingenious application of this proceſs to the analyſis of tin ores, according to the method which is deſcribed in his *Beitraege*: In this operation tungſtein is ſeparated from tungſtate of ammonia, by the addition of zinc, in the form of black flakes.

Alkaline ſolutions of metallic oxides precipitated, in the metallic ſtate, by other metals.

Experiments on falling Bodies, by M. BENZENBERG.

M. Benzenberg, profeſſor of phyſic and aſtronomy at Duſſeldorp, publiſhed, ſome months ago, twenty-eight experiments made with balls well turned and poliſhed, which were made to fall from a height of 262 French feet: At a medium they produced five lines of deviation towards the eaſt, according to the determination of the plumb-line, and the theory gives four lines ſix tenths. Theſe experiments were made in the coal-mines of Schebuſch; they are an additional proof, if it were neceſſary, of the rotatory movement of the earth, of which no one now doubts. The laſt experiments, made at Bologna by M. Guglielmini, gave nearly the ſame reſults.

A falling body deviates four lines ſix tenths eaſt, after paſſing through 262 feet.

Geography.

Great pains are taking in the conſtruction of an accurate map of Holland: The ſame precautions have been uſed in this buſineſs as in the meaſurement of the degree of the meridian. M. de Zach has publiſhed in his Journal the chart of the triangles which have been completed: They are joined to

Map of Holland.

thoſe which M. Delambre made for the great meridian; and the diſtance from Dunkirk to Montreal has been taken for the firſt baſe. When the triangles are finiſhed, a baſe will be meaſured towards the north, to ſerve for the verification of the work. The Batavian republic have entruſted the direction of this map to Colonel Krayenhoff.

Chart of the White Sea, by General Kahtauzoff.

Some months ago there appeared at Peterſburgh a very fine hydrographical chart of the White Sea, of which General Kautouzoff is the author: Many naval officers have worked under his direction for four years, in collecting the materials neceſſary to compoſe this chart. The coaſts of the White Sea, of its gulphs, and of a part of the Northern Ocean, have been laid down trigonometrically. The depths have been carefully ſounded; and ſix of the principal points of the coaſt have been determined by aſtronomical obſervations.

M. Lartigue's map of America in relief.

M. Lartigue having been engaged for thirty years in conſtructing, at the marine depot (of Paris), a large and beautiful map of America in relief, has at length completed it. It is ſaid that the mountains, and the iſlands, and the tints of the ſea, are all exhibited in a manner moſt capable of intereſting thoſe who make geography their ſtudy.

Expedition of Capt. Lewis up the Miſſouri.

Several months ago, Captain Lewis, in America, undertook to aſcend the river Miſſouri, in ſearch of a paſſage to the South Sea. Very intereſting intelligence may be ſoon expected from this expedition.

Survey of France.

The work of the government ſurvey, or *cadaſtre*, of France, has proceeded with activity; 2000 perſons are employed in it in the 108 departments.

Effect of Heat on Magnetiſm.

Magnetiſm deſtroyed at 700° of heat.

M. Coulomb has publiſhed an intereſting memoir on the effect of heat on magnetiſm. At 200 degrees of heat, two-fifths of it are deſtroyed, and the whole at 700 degrees.

A Secret Lock of ten Thousand Combinations.

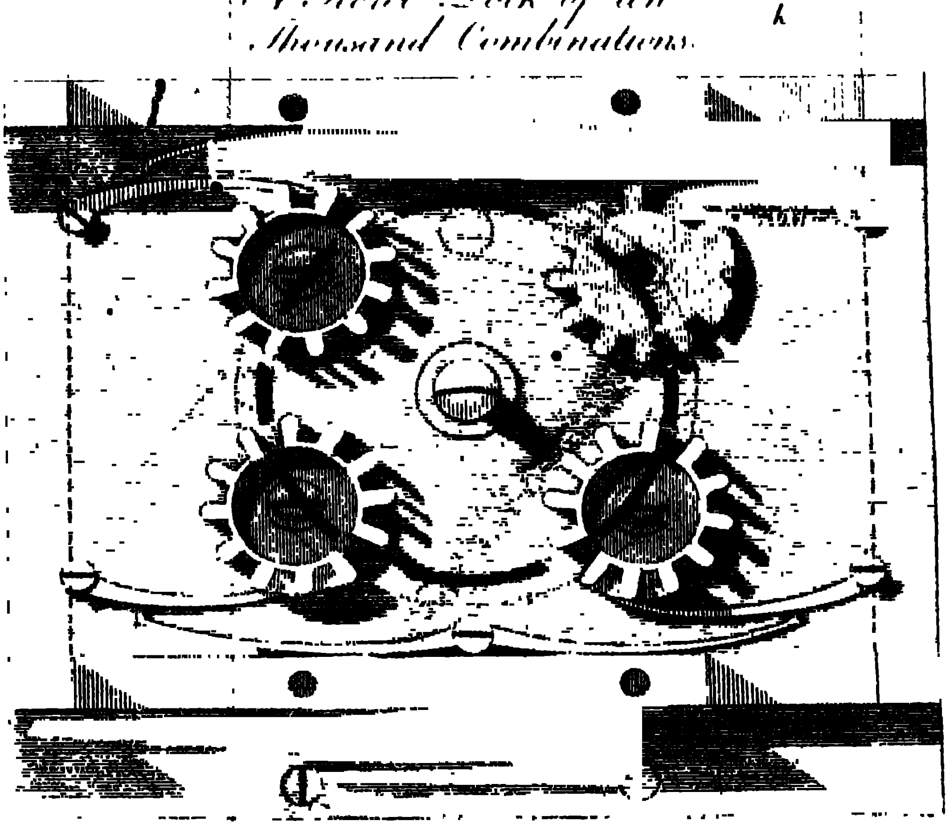
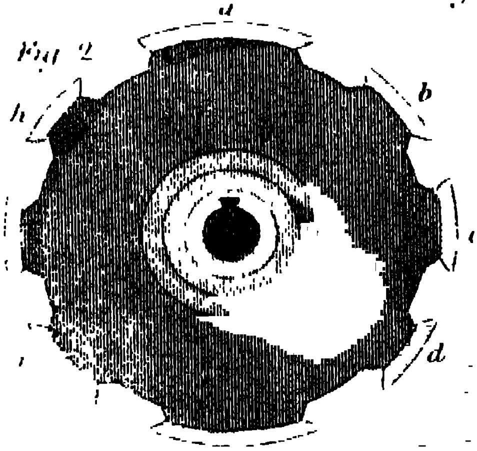


Fig 1

Fig 2



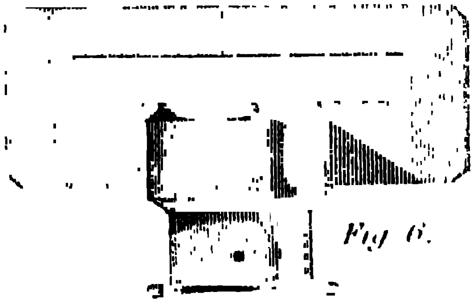


Fig 6.

Fig 3

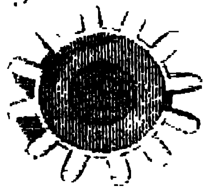
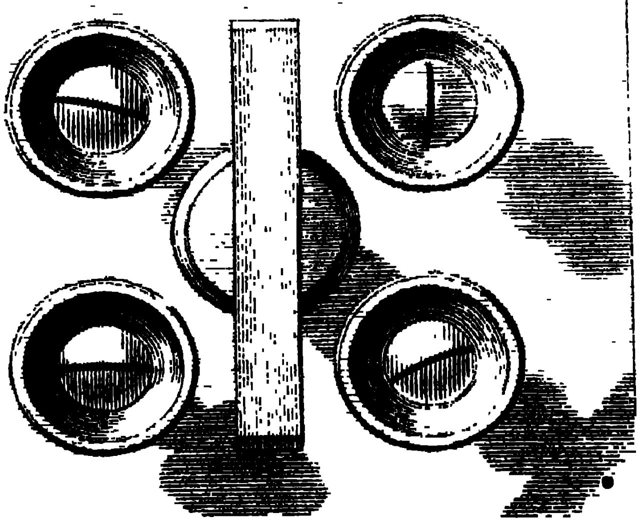


Fig 4.



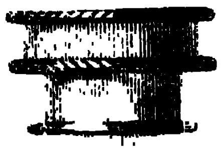
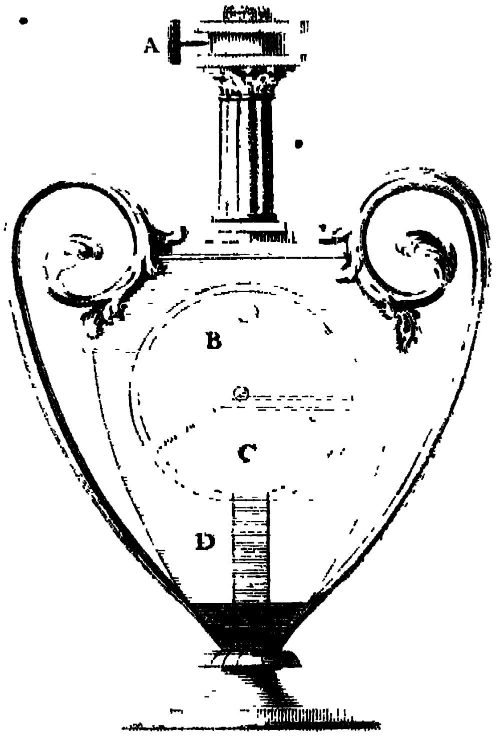
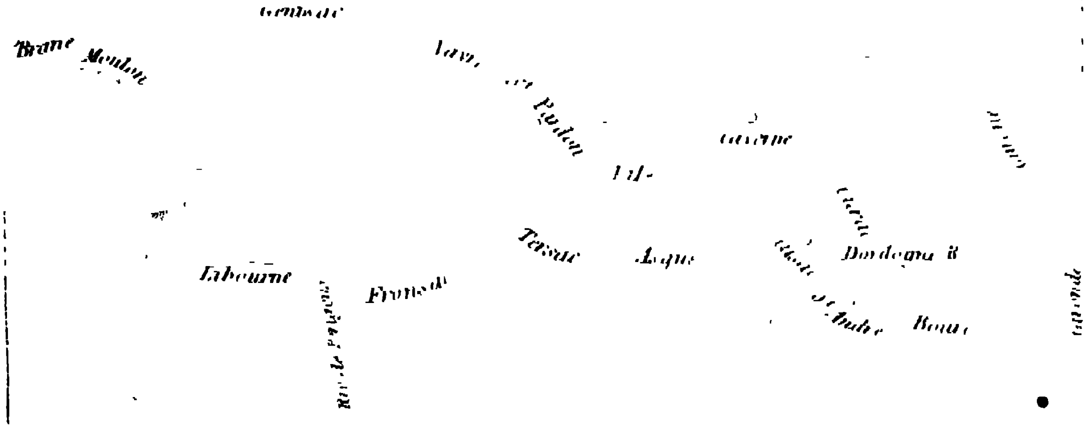


Fig 5

Mechanical Lamp





A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1806.

ARTICLE I.

Experiments on the Temperature of Water ſurrounded by freezing Mixtures. In a Letter from JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR, *Middleſhaw, Jan.* 29, 1806.

Expanſion of water in cooling below 41 deg.

MANY philoſophers have turned their attention to the dilatation obſervable in water when cooled below 40 or 41 degrees of Fahrenheit's ſcale, and alſo to the no leſs ſingular fact of water retaining its fluidity for a conſiderable time when expoſed to a freezing mixture, without being agitated. But one circumſtance, relating to the latter phenomenon, appears to have eſcaped the notice of them all; which in all probability will prove of ſome importance to both enquiries.

Explained by a developement of the conjecture that ice may be formed in minute cryſtals at that temperature.

We know from common experience, that when a hotter and colder body come into contact, the former will loſe and the latter acquire heat, until they arrive at an equality of temperature. The frequent opportunities every one has of making this obſervation have authoriſed it to paſs for a general rule; hence it has been concluded, that water in a ſtate of reſt may be cooled many degrees below the freezing point, and ſtill remain fluid. For my part, I adopted the maxim without heſitation, until the peruſal of Dr. Hope's paper, given in the ſupplement to your laſt volume, led me to reaſon in the following manner on the ſubject.

When water is expoſed to a freezing mixture, thoſe particles of it which are in contact with the ſides of the veſſel, are ſoon reduced to a temperature lower than the point of congelation; in conſequence of this, they are probably converted into minute icicles, which impart a quantity of caloric at the moment of their formation to the ſurrounding water, thereby preventing its temperature from ſinking below 32°. Theſe inviſible bodies afterwards begin to aſcend ſlowly on account of the diminution in their ſpecific gravity; and while they riſe towards the ſurface of the water, other particles will approach the ſides of the veſſel in ſucceſſion, and undergo a ſimilar tranſformation. This proceſs would evidently increaſe the volume of the water without reducing its temperature, ſuppoſing it to be ice-cold at the commencement of the experiment; for the hypotheſis reſts on the ſuppoſition that water freezes as ſoon as it is cooled below the 32nd degree of Fahrenheit's ſcale. This gradual increaſe of bulk will explain the appearances deſcribed by my friend Mr. Dalton, who found that thermometers filled with water continued to riſe when expoſed to freezing mixtures, until the encloſed water congealed ſuddenly, and frequently burſt his inſtruments. The reaſon why agitation accelerated the congelation of water thus circumſtanced appears to be this: When the inviſible icicles become very numerous, the leaſt motion carries them in crowds againſt the ſides of the veſſels; where the ſmall quantity of water contained amongſt them cryſtallizes immediately, and cements the whole into a film adhering to the inſide of the cup. This theory, or hypotheſis, call it whatever you think proper, evidently requires water to be what it really is, namely, a bad conductor of heat; and after forming it, I proceeded to examine the merits of it experimentally, in the following manner.

Experiment. Water at 32° was cooled by a ſurrounding mixture, and became congealed at the ſides by ſtirring, having never ſunk below 32°.

Experiment 1ſt. A ſmall thermometer was ſuſpended at the lower end of a wire, which could be moved in a vertical direction, through a hole in a horizontal bar of wood, fixed over a table for the purpoſe; a veſſel, containing a freezing mixture, of the temperature of 21°, was next placed with its centre under the wire; and the bowl of a wine glaſs, filled with two ounces of ice-cold water, being then properly placed in the mixture, the thermometer which ſtood at 32°, was immediately let down into the water, where it remained ſtationary for the ſpace of ſeven minutes. A wire, cooled to the freezing point was now introduced into the glaſs, and the water agitated with it; upon which a thick coating of ice formed on the inſide of the veſſel; but no marks of congelation were obſervable on the wire or thermometer.

Exp. 2. The ſame apparatus being uſed, with a mixture, having the low temperature of 6°, the glaſs was filled with water of 58°, in which the thermometer fell to 32° in 7¾ minutes, by a ſtop watch; at which point it remained ſtationary five minutes longer. The glaſs was then taken out of the mixture, and the water being agitated, lined the upper part of it for about two-thirds of its depth from the brim, with a porous covering of ice, but the remaining part of it was free from all incruſtation.

Exp. 2. Water at 58° was cooled by an intenſe freezing mixture. It was brought to ſtationary 32° and ſunk no lower. When taken out and ſhaken the top froze.

I will venture to infer from the two preceding paragraphs, that we have all been under a miſtake in concluding that water may be cooled when at reſt many degrees below 32° of Fahrenheit, without congealing; at the ſame time we are certain, that it will preſerve its fluidity, when judiciouſly expoſed to great degrees of cold, and dilate at the ſame time, as Mr. Dalton has proved. Now as the heat never falls below 32° in theſe experiments, the expanſion of the water in Mr. Dalton's thermometers, placed in a freezing mixture, cannot be aſcribed to a loſs of temperature, but muſt be owing to ſome other cauſe, probably to that which has been aſſigned above. As for agitation, the firſt experiment ſeems to ſhew its office to conſiſt in bringing the water, crowded with minute icicles, into contact with parts of the veſſel much colder than itſelf, where it is concreted into ice.

Hence water cannot be cooled and remain fluid at temperatures below 32°, as generally ſuppoſed. The water thermometer muſt expand in cooling by ſome other cauſe.

Exp. 3. To examine this part of the ſubject with more care, I formed a cup of caoutchouc, the capacity of which for caloric greatly exceeds that of glaſs; or, I believe, that of moſt other ſubſtances. Two ounces of water, a little warmer than melting ſnow being poured into this cup, it was placed in a mixture of the temperature of 15°, where it remained eight minutes without giving the leaſt indication of a tendency to freeze. The cup was now removed from the

Exp. 3. Repetition of Exp. 2 very ſtrikingly in a cup of caoutchouc.

mixture, and gently ſhaken; upon which long icicles formed in an inſtant, projecting into the water in all directions, from the caoutchouc to which they adhered. This experiment, I have no doubt, might be made a very beautiful one by a dexterous operator, who is in the habit of exhibiting natural appearances to public aſſemblies.

After diſcovering that water will dilate without any change of temperature from warm to colder, at 32°, I began to imagine that the whole variation of expanſion under 41°, might be explained on the ſame principle, becauſe I believe all the experiments relating to the ſubject, have been made in a cooling medium, not warmer than melting ſnow.

Water expands by cooling between 41 and 32 deg. or begins to cryſtalize at the upper term.

In order to try the merits of this opinion, with an inſtrument larger than a common thermometer, I filled a four-ounce phial with water, and fixed an open tube into it, by means of a perforated cork and cement; but this apparatus proved my ſuſpicion to be falſe. For the place of the water being marked on the tube when the temperature was 41°, my bulky thermometer roſe immediately upon being plunged into water of 34°. This fact proves, that water expands by a loſs of temperature between 41° and 32°; or elſe, that this fluid begins to cryſtalize at the upper term; in conſequence of which the lower term, or 32°, is not, properly ſpeaking, the commencement of congelation, but the point at which the cryſtals of water begin to concrete into maſſes by aggregation.

I remain, &c.

JOHN GOUGH

II.

Account of the Art and Inſtruments uſed for boring and blaſting Rocks; with Improvements. In a Letter from G. C.

To Mr. NICHOLSON.

SIR, *Briſtol, Jan.* 21, 1806.

Improvements in blaſting mentioned.

BY way of appendix to Mr. Cloſe's remarks on the uſe of ſand in ſtemming mines in hard rocks, and his uſeful improvement of the pricker, by making it of copper inſtead of iron, allow me to add two other improvements in the art of blaſting ſtone, which my own experience has proved to diminiſh conſiderably the expence of gunpowder, while one of them, at the ſame time, removes all danger from imperfect priming.

I ſhall alſo, with your permiſſion, as many of your correſpondents muſt neceſſarily be ignorant of the conſtruction of the tools, give you a deſcription of thoſe now in uſe at the village of Shipham, in Somerſetſhire, a village wholly compoſed of men, women and children, who mine after lead ore, calamine, and ochre, chiefly in a lime-ſtone rock; a numerous band of ſome of the ſtouteſt beings in England.

Deſcription of the art as practiſed in the lime ſtone rocks in Somerſetſhire.

Theſe men ſtill uſe the iron pricker, becauſe an accident ſeldom or ever happens to them; owing, I believe, in a great meaſure to their ſtemming with ſpar, and their habit of turning and looſening the inſtrument at every half inch they fill.

The iron pricker uſed without bad effect with ſpar for ſtemming.

The tools they uſe are theſe, *Plate* V.

A. A round bar of iron, bevilled off at one end, of 18 inches long, and of the diameter of half an inch.

B. A ditto, of 24 inches, to follow when the hole in the ſtone is about 12 inches deep.

C. A rod, with a loop for the finger, 25 inches long; at the bottom of which is a round flat plate of iron to draw out the pounded ſtone occaſionally.

D. A pricker, 24 inches long, with a loop alſo, uſed to preſerve a paſſage to inſert the priming ſtraw, while the hole is rammed or ſtemmed with E, the iron rammer, 20 inches long, and which, ſix inches or more from the end, is formed into a conical groove, very open at bottom, in order to enable the miner to ram round the pricker, and alſo that by its ſharpneſs at the end it may the eaſier break to duſt the pieces of ſpar dropped in as faſt as wanted.

Tools and implements deſcribed. A hole eighteen or twenty inches deep and half an inch in diameter is cut by repeated ſtrokes of a chiſſel, the edge of which lies in the diameter of the hole and is ſhifted round between ſtroke and ſtroke.

F. A hammer with a handle and ſtrap, about five inches long; the iron head weighing about four or five pounds, according to the ſtrength of the operator; for ſome have them of ſix or ſeven.

They alſo have by them a bottle of water, to pour occaſionally into the hole, for the wetter it is the faſter the work goes on.

At every ſtroke of the hammer, the miner turns his chiſſel, by which means he works the bottom of the mine in a regular circle, and is enabled to keep his perforation true.

The work is performed wet, and the chipped ſtone ſcooped out with an inſtrument.

When arrived at the depth of 18 or 19 inches, he cleans, The charge of powder is an ounce (which is too much). An iron wire called the pricker is put down in the hole cloſe to the ſide, and ſmall pieces of ſpar are dropped in, which are ſlightly rammed and afterwards more firmly. The whole being full, the pricker is drawn, and a wheat ſtraw filled with gunpowder is put down in place of the pricker.

and, as well as he can, dries his mine; then inſerts his charge of gunpowder, often amounting to the unneceſſary quantity of an ounce, and dropping the pricker to the bottom, with its ſide touching the ſide of the mine, he begins by dropping into it ſome lumps of ſpar; and after he has filled up about an inch, begins pounding it round the pricker with his rammer and hammer; tapping gently at firſt, but ſoon beginning to ram very hard, all the while frequently turning and looſening the circular pricker.

When the hole is quite filled, he draws it, by giving ſome gentle ſtrokes on the chiſſel that he has now paſſed through the loop to draw it with.

He then takes the upper joint of a wheat ſtraw, the ſmalleſt he can get, and having ſtopped the fine end with clay, if it has no knot; he afterwards places the other end, cut off very bevel and ſharp, between his ſecond and third finger of the left hand, cloſe to where the fingers join the palm, forming his hand into a kind of baſon, to keep off the wind, and drawing the open end of the ſtraw ſo low between the fingers that he can but juſt prevent it from dropping on the ground; when pouring a ſmall quantity of gunpowder on the orifice, and tapping with his other hand on the ſtraw below, to ſhake it, it ſpeedily is filled.

This ſtraw muſt be 19 inches long for a hole of 18 at leaſt, and a little ſhaved away at the bottom, but not cut open of courſe.

Fire is given by a piece of touch-wood,

When thruſt down to the powder the train is compleat, and our operator laſtly lights a piece of touch-wood, and places it ſo that when all on fire, it ſhall communicate to the train; after which he withdraws to a place out of the line of exploſion, and waits its effect.

—which occaſions loſs of time by its failure and danger, when too rapid.

And here in blowing a well, I found that much time was loſt; for not only does the wind occaſionally blow away the touch-wood before it is all inflamed, but frequently the damp extinguiſhes it. I alſo found there was danger to the workman if it went off too ſoon, which the wind ſometimes occaſions, or his companion is too ſlow in haling him up; and we likewiſe found that when they worked by the day, and *we* found powder, they uſed an immoderate quantity.

Improvements. If a cork be thruſt down the hole or well pre-

To remedy theſe two great evils, I purſued the following plan: the firſt of which was ſuggeſted to me by an ingenious neighbour and both had the deſired effect.

The firſt experiment I tried was upon a ſingle block of limeſtone, of about two ton weight. I charged the mine with only the common charge of a muſket, as at K, over which I drove a cork, as at H, leaving one inch, or thereabout, as at I, over which I rammed ſpar, as at G, up to the ſurface of the rock.*

vious to ſtemming, ſo as to leave one inch of windage, leſs powder will be requiſite.

I then made a ſlit in my ſtraw train, as at L, and paſſed through it, as through a loop, a cut of the German aſh-tree fungus; but not liking that, as endangering the loſs of the priming powder, I cut the ſlit in the fungus, as at N, paſſing the ſtraw through the ſlit, and cutting a ſmall notch on one ſide of the ſtraw, as at O. When it was ſlid down to it, being elaſtic, it cloſed there, and filled the notch.

The German fungus or amadou is ſteadier and more certain than touchwood. The ſtraw may be thruſt through a hole in this fungus,

This match burns ſlow but ſure, and no wind can extinguiſh it. A great advantage, as I have frequently witneſſed, in making the new and beautiful towing path on both ſides the Avon, from Briſtol. One hundred men loſe from ten minutes to twenty and more while getting out of the way during the blowing of a mine near the ſpot they were levelling, and all owing to the ſlow burning of the touch-wood match, or the wind blowing it aſide.

—which burns ſlowly, but with certainty, and is not blown out by the wind.

This German match is, I fancy, pretty well known; it is merely the fungus of the aſh-tree, macerated and hammered until it becomes as flexible as a piece of buff leather, and has been called the German match, I believe, from its general uſe on the upper Rhine, where, by its means, habitual ſmokers of tobacco can light their pipes in the open air, whatever may be the weather; and as a piece which ſcarcely weighs four grains is ſufficient to light without danger, the largeſt mines, while the article is by no means dear, and always ſafe, inextinguiſhable, and regular in its burning, nothing can be more uſeful to the practical miner.

Account of the fungus.

With reſpect to ſand (which I ſee recommended in a Dublin paper of only laſt week, as a new diſcovery in ſtemming), it will not always ſucceed, eſpecially in thoſe great mines of the Clifton blaſters, where, often 15 or 16lb. of powder are uſed at a time; but I ſhould think if ſtopped with a ſtiff clay, it would greatly encreaſe the reſiſtance, eſpecially if ſufficient windage was left over the powder.

Stemming with looſe ſand will not (it is thought) ſucceed in mines upon a large ſcale.

The experiment I firſt tried, as deſcribed above, on that plan, tore to pieces, and threw four pieces of my rock to a

The author's experiments were fully ſucceſsful.

great height, ſhaking and leaving fit for loading, a good cart load in all; while, but for a wall at hand, my Shipham miner, as uſual, deſpiſing novelties, would probably have been wounded, having been with difficulty perſuaded to take that cover at four yards diſtance.

Thus, Sir, I have ſtated what I take to be improvements in this valuable art, and if they afford you or your readers any gratification, I ſhall not regret the trouble of putting them on paper, being always, Sir,

Your grateful reader,
G. C.

III.

Deſcription of a new Parallel Rule, exempt from lateral Deviation; invented by Mr. J. W. BOSWELL; *with an Account of the Imperfections of thoſe already made for the ſame Purpoſe.*

To Mr. NICHOLSON.

DEAR SIR,

Inconveniencies from the lateral deviation of the common parallel rule.

THE common parallel rule of four pieces has been long found inconvenient, on account of the lateral deviation of the moving piece, which cauſes a neceſſity of ſhifting the poſition of the whole rule frequently, when many parallel lines are to be drawn; that, beſides the loſs of time which it occaſions, tends alſo to produce error in the paralleliſm of theſe lines.

It is not ſuperior to the triangular plane and rule.

For this reaſon it is in no reſpect ſuperior to the more ſimple apparatus for the ſame purpoſe, formed by a triangular plane of wood or metal, moved along a common rule; and as this latter is more ſteady, and ſerves for other purpoſes in drawing, it is preferred by ſeveral.

Parallel rules as yet contrived to operate without ſide deviation are ſubject to inaccuracy,

Many inſtruments have been contrived to draw parallel lines without being ſubject to the imperfections here ſtated; but all, that I know of, are more or leſs deficient in correctneſs, from requiring an exactneſs in their formation hardly attainable, or from extreme tendency to have this perfection deranged when attained.

The parallel rule with ſliding joints difficult to make exact, and

The parallel rule with croſſing connectors, and two ſliding joints, is ſubject to both the above inconveniences. The leaſt play in the ſlides, or deviation in the grooves in which they are to move, muſt alter the paralleliſm of the lines drawn by it; and however exact it may be at firſt, the natural wear attendant on its uſe muſt demonſtrably produce theſe imperfections; to which may be added, that the nicety of workmanſhip which it requires, and its complicated form, muſt of courſe render it expenſive.

becomes inaccurate from wear.

The inſtrument formed by a rule ſupported by two ſmall wheels fixed to the ſame axis, which axis is placed ſo as to be parallel to the edge of the rule, is liable to be imperfect, from any difference in the diameters of the two wheels, or ſlight inaccuracy in the poſition of the axis.

This rule is alſo very liable to ſlip on the paper, and is rendered incorrect in its effects by any unevenneſs in the ſurface over which it is moved.

That moving upon two wheels fixed to one axis, has the ſame defects, and is liable to inaccuracy from the unevenneſs of the paper.

The parallel rule, mentioned in your ninth volume, page 212, requires an exact proportion in the length of each of its parts; and as theſe are all of different meaſures, would be liable to error in the firſt formation, on this account; and however exactly made, would, after a little wear, ſoon deviate, on account of the play which this would produce in the joints; the connectors alſo between the two rules, paſſing from different extremities, and leaving long ſpaces beyond the points of ſupport, would thereby occaſion any play in the joints to produce a greater deviation from paralleliſm in the lines drawn.

That formed by ſeveral unequal joints difficult to make exact, and liable to become very incorrect in wear from its long projections beyond the points of ſupport.

The apparatus for producing parallel lines formed by the drawing board and normal ſquare, can hardly with propriety be claſſed among the inſtruments here treated of; whatever its accuracy may be, its cumbrous form, and the time required for faſtening the paper to it, render it for many purpoſes very inconvenient.

The drawing board and normal ſquare is cumbrous, and waſtes time.

Theſe conſiderations induced me, about the time when the account of the parallel rule, given in your ninth volume was publiſhed, to conſider how a parallel rule might be conſtructed not liable to ſide deviation, and as free as poſſible from the defects of the others above ſtated. The inſtrument which then occurred to me as the beſt calculated for this purpoſe, I ſhall now deſcribe; and as I have often examined it ſince, if it poſſeſſed any material defect, it is probable it would have become manifeſt before this; in which caſe I ſhould not have brought it forward to public notice.

Deſcription of Mr. Boſwell's parallel rule to prevent lateral deviation.

My inſtrument for drawing parallel lines without ſide deviation, is formed of three rulers, laid parallel to each other, connected by two pair of moveable pieces, all of equal length, and parallel to each other; theſe pieces, where they meet on the middle rule, have their extremities formed into portions of toothed wheels, which lock into each other, as may be ſeen in the figure: the effect of theſe ſegments of wheels thus acting in each other, is, that all the lateral motion is transferred to the middle rule, while the external rules move only in an oppoſite and parallel direction.

The contrivance to prevent lateral deviation cannot affect its accuracy. Its ſupport at each end makes it ſteady. It is eaſily made exact.

This inſtrument will not be liable to the incorrectneſs of thoſe before deſcribed, for the following reaſons: 1ſt. The toothed ſegments being in no way concerned in producing the parallelism of the inſtrument, its accuracy of parallelism cannot be at all affected by any trifling incorrectneſs of formation in their parts. 2nd. All the connecting pieces being of equal length, can be formed with more certain accuracy. 3d. The connecting pieces paſſing from the ſame extremities of the external rules, give them a ſteady ſupport. For theſe reaſons, in my opinion, it poſſeſſes all the ſteadineſs and facility of formation of the common parallel rule, while it effectually prevents the ſide deviation, to which the latter is liable.

It might be made with but one pair of toothed ſegments, but two pair make it look more uniform.

It is not abſolutely neceſſary to have more than one pair of the connecting pieces made with toothed ſegments; but as theſe ſegments are eaſily formed in the clock makers engine for cutting teeth in wheels, it can add little to the expence to make the two pair in this manner, as ſhewn in the figure, and will make the inſtrument look more uniform.

The middle rule ſhould be made thinner than the other to prevent friction.

The middle rule ſhould alſo be made a little thinner than the others, to prevent friction on the paper in its lateral movement when in uſe.

Novelty of the inſtrument conſiſts in its toothed ſegments.

In the deſcription of this inſtrument, it will be obſerved, that the novelty of it conſiſts in the application of the toothed ſegments of the wheels to the uſe mentioned; which I cannot find has ever been before uſed for this purpoſe; and I think it highly probable it has not, as, beſides its not being known to gentlemen, whom I have conſulted on this head, moſt likely to be acquainted with ſuch matters, the ſimplicity of the contrivance would probably have brought it into extenſive uſe, if it had been ever known at any former period.

Reaſons for ſuppoſing this invention to be new.

I mention this only to ſhew that, before I claim the priority of invention, I have taken ſome pains to inveſtigate my pretenſions;

tenſions; which I think is incumbent on every man to do on ſuch occaſions: for, however fair the claim may be of invention, if a thing is well known to have been before done, it at leaſt produces an aukward ſenſation to the claimer; for which reaſon, thoſe who accuſe others of doing this, ſhould be the more cautious, that their accuſation is fair in all its parts; for oftentimes an external reſemblance may ſubſiſt between two contrivances, as between my inſtrument and the triple parallel ruler, and yet a ſmall addition render their effects eſſentially different; thus the triple parallel ruler admits of ſide deviation, while my parallel ruler effectually prevents it.

A ſmall addition to an inſtrument ſometimes renders its effects eſſentially different.

My motive for publiſhing the account of this inſtrument is principally becauſe I think it a duty incumbent on every man, who has contrived any thing that may be of uſe to the world, to make it known as extenſively as poſſible, which it certainly will be by appearing in your Journal.

The inſtrument from which the figure was drawn was made according to my directions, by Mr. Banks, inſtrument maker, No. 441 in the Strand, and anſwers the purpoſe perfectly well; of courſe any gentlemen who deſire to uſe parallel rulers of this kind, may have them accurately made at the ſame place.

This inſtrument made by Mr. Banks, 441, Strand.

I requeſt the favour of your permitting the inſertion, at the end of this communication, of the indication of ſome typographical errors, made in my paper relative to the performance at ſea of the ſhip Economy, in your laſt number; and which I am the more anxious to have rectified, as ſome of them entirely alter the ſenſe of the paſſages where they occur.

Typographical errors in Mr. Boſwell's laſt paper relative to the ſhip Economy.

Page 175, line 2, eraſe *it* before *could*; line 6, transfer the bracket to before *when* in the next line; line 8, for *is* read *are*; eraſe the comma after *is*; and transfer the bracket to after *proof*; line 9, 10, for *direction* read *directors*; page 176, line 15, for *ſource* read *purſe*; line 21, eraſe *not* before *reſt*; page 179, line 22, for *point* read *front*; line 37, for *forms* read *frames*; page 180, line 3, for *ſcarping* read *ſcarfing*.

Some errors of the preſs are alſo apparent in the ſide notes, but I ſhall not trouble you by pointing them out, as they can be rectified by the meaning of the paſſages to which they are added. I am, Dear Sir,

Your very reſpectful humble ſervant,

J. W. BOSWELL.

Reference to the Figure, Plate V, Fig. 2.

A A,A A. The external parallel rules, B B the central rule, C D,C D the connecting pieces, D D the ſegments of toothed wheels in which the connecting pieces terminate, which by their action on each other prevent ſide deviation in A A,A A.

IV.

Letter from an ENQUIRER, *on the Waſte of Fiſh aſſerted to be made on the Scottiſh Coaſt. In Reply to A. L.*

To Mr. NICHOLSON,

SIR, *London, Feb.* 7, 1806.

Proper ſpirit of inquiry and publication.

I AGREE moſt cordially with your Correſpondent A. L. of Aberdeen, in page 168, with regard to the accuracy of important information when communicated to the public, and that when doubts exiſt, it ſhould be given with ſo much modeſty and diffidence, as to ſhew that the communicator is not certain of his ſubject. Of the ſtatement I made reſpecting thoſe inſtances of waſteful negligence in ſome fiſheries of the north of Scotland, I am not the firſt; the reſpectable author of the ſtatiſtical account of the pariſh of Peterhead, the Rev. Dr. Moir, has aſſerted the ſame, limited to that pariſh that I did. In the 16th Vol. p. 550 of that work, he ſays, "turbot (I believe the holybut of the London market) is now caught frequently, and in great perfection. Thirty years ago they were ſeldom uſed here, *frequently caſt into the dunghill*, or *left to waſte on the ſea beach*, they at preſent ſell from four pence to one ſhilling each, and are riſing every day in price;" in the preceding page of that volume we are informed, that "the greateſt part of the *cods' ſounds*, in this pariſh, are *permitted to remain and rot on the ſea beach*, or, *are caſt into the dunghill*, though the uſe and value of them as an article of food and delicacy at table have been known here for many years," and yet in the following paragraph the Doctor tells his readers, "that the crews of the ſhips have been ſent from this town to Barryhead, *to preſerve the ſounds*, tongues, and palates of the cod caught *there*, and the owners have always

Scotch fiſheries.

found a ready market for them!" for myſelf, Sir, before I even hinted through your Journal, at theſe ſtrong affirmations, I made it my buſineſs to enquire of ſome friends at Aberdeen, of the truth or falſehood of ſuch aſſertions; deeming it then, as that gentleman does at preſent, an improbable ſtatement; under theſe circumſtances I cannot conſider the communication you did me the favour to inſert, as militating either againſt your correſpondents rule of examination, or my own habitual ſcepticiſm:—that gentleman, in recommending accuracy of ſtatement, ought not to have forgotten it himſelf; he will eaſily ſee that *Aberdeen* is not mentioned by me as being at all concerned in this waſte of ſuſtenance. My little note to you has rouzed the attention of A. L.—Is it wandering too much into

"*the fairy regions of romance,*"

to hope that the ſubject may obtain ſtill farther notice? and continue to do ſo until it be made productive of *all the advantages it is capable?* in that caſe, ſuppoſing defective information in my firſt notice of it (and I preſume A. L. will allow I had ſome authority for my opinion, and that he himſelf has not been completely accurate) my errors will be eventually attended with good. Your correſpondent well knows, that the aſſertion of Dr. Johnſon about the ſcarcity of trees in Scotland, has had the happieſt effects. How far that gentleman's queſtion about the vend at the towns I mentioned, may be anſwered in the affirmative or otherwiſe, I have not yet ſufficiently informed myſelf; but certainly under the circumſtances I conceive to be true, thoſe markets would be preferable to ſuch waſte. The men employed may look with confidence for a ſpeedy ſale; and, if I am not very much miſtaken, theſe towns are ſupplied from the Yorkſhire boats, the wind therefore which brings the one ſet of boats, would impede, if not totally hinder the other. Far am I from wiſhing to throw any obſtacles in the way of ſo excellent a plan, as that for a ſociety for exporting white fiſh from Aberdeen, but where would your correſpondent ſend them to? "*is it not to be ſuppoſed, that fiſhers of the places neareſt to ſuch towns could greatly underſell them?*" this queſtion is not a greater difficulty in the ſcheme ſuggeſted by me of bringing the fiſh to Leith, Berwick, or Newcaſtle, than it is to that of A. L. The fact is, that by giving that queſtion weight, competition, in every buſineſs

Good effects o
inquiry and
public remark

buſineſs would be undone; for my own part, I do not think it any objection to either of our ſchemes; nay, our plans conſidered ſeriouſly, are nearly alike. I ſuggeſted, rather I believe in the form of a query than otherwiſe, the propriety of bringing fiſh where I know a demand exiſts; but that gentleman oppoſes my ſuggeſtion with the above queſtion, and then propoſes a plan on a ſimilar, but more comprehenſive ſcale! Allow me merely to correct at preſent one more error A. L. has, *unintentionally, I am certain,* committed in the laſt paragraph of his vindication of the Arbroath fiſhers (of whoſe methods, and *probable* waſte, I hope ſoon to obtain a *correct* account,) It is *not* the caſe even for the moſt part in *every* large fiſhing town, that the fiſhermen " retain the bodies of the crabs, and ſell the large claws only. A liſt of ſome of the towns in which that cuſtom prevails, would, doubtleſs oblige many of your readers. If I am correct in my opinion of A. L. all *his* attempts are for the ſpread of uſeful knowledge; his candour will ſuggeſt the propriety of viewing mine alſo in a favourable light.

I am, Sir,
Your's and A. L's,
Friend and Servant,
AN ENQUIRER.

Notice of a Publication of Importance intended by the Literary and Antiquarian Society of Perth.

SIR,

IT is with great pleaſure I inform you that the very reſpectable Literary and Antiquarian Society of Perth, intend giving a ſelection of their valuable papers to the public; it is much to be lamented that they have delayed doing this ſo long, as many of their manuſcripts throw a very extenſive light on the antiquities of that part of the iſland.

I am, Sir,
Your's truly,
N. L.

To Mr. Nicholſon,

Newcaſtle-upon-Tyne, Jan. 28, 1806.

Letter concerning a Library eſtabliſhed at Aberdeen. From a TRAVELLER.

SIR, *York Hotel, Bridge Street, Blackfriars.*

I AM extremely glad to find, that there is a ſubſcription library eſtabliſhed in Aberdeen. I am aſtoniſhed, however, to be informed from Mr. Crombie's paper, that none of the very learned Profeſſors in that part of the country are engaged in the undertaking! perhaps if the ſubſcription was raiſed to one guinea per annum, much more good might be effected, and thoſe gentlemen would not then ſcruple to join themſelves; their freedom as to pecuniary motives is well known. I hope for the ſake of the general diffuſion of knowledge, to find myſelf equally miſtaken with regard to Banff, Peterhead, and Inverneſs. The ſociety at Aberdeen, though young, ſeems to be conducted with great liberality, as appears evident from their offer of aſſiſtance to the places above mentioned, or any other that may be now forming rules; is it too much for a friend to the ſpread of uſeful knowledge to ſuggeſt to them an extenſion of the benefits of their aſſociation, to thoſe gentlemen who are members of ſimilar ſocieties, whilſt in Aberdeen, on condition of a return of ſuch civilities, ſhould any of their members be where ſuch libraries are? a rule of this kind you Sir, have mentioned with applauſe in a former number,

Your's, &c.

A TRAVELLER.

February 7, 1806.

V.

A Chemical and Medical Examination of the Gizzards of White Fowls compared with Gelatine, together with an Expoſition of the Characteriſtics of the latter when oxigenated, By M. BOUILLON LAGRANGE *.

IT has long been underſtood, that the gizzards of white poultry poſſeſſes certain medicinal qualities. The uſe made of it by many phyſicians may juſtify ſome reliance upon the

Gizzards of fowls medically uſed.

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* *Annales de Chimie*, Vol. LV.

virtues attributed to it; but no one, I believe, has hitherto thought of analyſing this ſubſtance.

It occurred to me, that it would be uſeful to the art of healing, were a few chemical facts added to the knowledge already poſſeſſed of the medical uſes of gizzard, particularly after reading in the "*Journal d'Economie Rurale and Domeſtique, ou Bibliothèque des Propriétaires ruraux, Pluvoiſe, an* 12;", a letter, wherein is announced the ſucceſs obtained by its uſe in agues. As this letter contains the details of the preparation, and adminiſtration of this remedy, I ſhall tranſcribe it at length.

" *Amiens, 25 Frimaire.*

Letter reſpecting it. Recommended as a febrifuge by the French government.

" YOU mention animal gelatine as a febrifuge, I will inform you of a more ſimple and leſs expenſive remedy. I know not by what fatality this great ſpecific has been neglected, notwithſtanding it was publiſhed by government full forty years ago, and in ſpite of its efficacy, of which I have had long experience; for, of about a thouſand caſes, in which during that period, I have adopted its uſe, I can atteſt the cure of eight-tenths.

" I have reſided at Montpellier during fifty-ſix years; the climate of the place and its environs is mild and ſalubrious; but the inhabitants along the coaſt are ſubject to agues, on account of the vicinity of the Mediterranean, and of ſtagnant pools, M. de St. Prieſt, intendant of this province, publiſhed the order of government relative to the remedy above alluded to.

Preſcription.

" *Remedy.*—This remedy conſiſts of the gizzard of fowls, dried and pulverized.

The gizzards are waſhed, dried, and pulverized.

" *Preparation.*—Take the gizzard of white poultry, as fowls, turkies, &c. (I never made uſe of thoſe of black fowls, as pigeons, ducks, &c.) open them, and clear away the gravel they contain; having ſlightly waſhed them, let them be put on a ſtring and hanged in the ſun, or up a chimney to dry, after which they muſt be reduced to powder, ſifted, and kept in a bottle cloſely corked.

" *Doſe.*—The doſe is about a drachm for adults, and from half a dram to a ſcruple for children.

The doſe is one drachm taken in wine.

" *Mode of taking.*—Mix the proper quantity of the powder in a glaſs or half-glaſs of good old white wine, and let the patient ſwallow it about half an hour before the fit comes on, or on the appearance of the precurſory ſymptoms of the fever. This being thrice repeated, it rarely happens that the diſorder returns.

" *Regimen.*—A wholeſome regimen is all that is neceſſary during the adminiſtration of this medicine, but the patient ſhould carefully avoid expoſure to moiſture or cold, particularly in the feet."

The foregoing details lead us naturally to the following obſervation:

Queſtions whether it be gelatine.

Should this ſubſtance be conſidered as gelatine, and poſſeſſing the ſame property of being a febrifuge, as ſtated by M. Seguin; or ſhould we rather acknowledge it to be poſſeſſed of thoſe particular virtues which have been attributed to it by ſeveral emient phyſicians? M. Pia, an old apothecary of Paris, aſſured me, that full thirty or forty years ago, the powdered gizzard of poultry was recommended in all obſtructions of the urinary paſſage, in complaints of the bladder occaſioned by ſlimy matter, as well as in all nephritic pains.

Its efficacy.

" The efficacy of this remedy has long ago eſtabliſhed its uſe;" and the writer adds, " that during my practice in pharmacy, I have prepared large quantities of it; ſo much were phyſicians and their patients ſatisfied with its operation."

they ſhould not be dried in the ſun.

His method of preparing the gizzards was to chooſe thoſe of young fowls, and particularly of pullets: after cleanſing, rubbing, waſhing, and wiping them carefully, he ſtrung them, and left them to dry on hurdles between ſheets of paper, aſſiſted by the gentle heat of a ſtove, and not in the ſun, which, according to M. Pia, would have ſpoiled them.

Vitreous appearance.

When the gizzards were properly dried, they became friable, almoſt tranſparent, and exhibited on being broken a vitreous appearance.

Powder aſh-coloured, mucilaginous and ſaltiſh.

The powder obtained was of a whitiſh grey aſh-colour, yielding in the mouth a kind of mucilage, and poſſeſſing a ſlightly ſalt and bitter taſte.

Doſe.

This powder was adminiſtered twice a day, (morning and evening) in doſes of twenty-four to thirty-ſix grains, in a glaſs of the infuſion of pellitory of the wall; of bearberries (*uva urſi*) or of lintſeed ſweetened with ſyrup.

Effects.

The efficacy of this remedy as a diuretic and aperient, was ſo much relied on, that the afflicted even omitted the infuſion and took it in pure water with a little ſugar.

Arguments in its favour.

The long experience which has been had of the ſalutary effects of gizzard as a febrifuge, diuretic, aperient, &c. and the publicity which the government, doubtleſs not upon light grounds, has given to this remedy, are authorities in its favour; and it muſt therefore be an acceptable labour to the phyſician, to furniſh him with new lights upon an object ſo eſſentially intereſting to humanity. This is the motive by which I have been induced to ſubmit the following experiments to the ſociety of medicine.

Inquiries as to its compoſition and uſe.

As gizzard has a great analogy to gelatine, I endeavoured to diſcover their ſimilitude. If gelatine be really a febrifuge, gizzard ſhould be ſo likewiſe, particularly as it contains, when freſh, a large portion of that ſubſtance; but whence does it derive its power as a diuretic, aperient, &c.? does it poſſeſs it in common with gelatine? I cannot tell. Or, have the ſaline parts of its compoſition this double property? of this alſo I am ignorant; for practice has not yet aſcertained whether the anti-febrile quality ſhould be aſcribed to the acidulous ſalts rather than to the ſubſtances with which they are combined.

Experiments on recent gizzard.

A freſh gizzard preſented the following phenomena.

A. The water wherein this ſubſtance had been boiled acquired a yellowiſh white colour, and flakes were depoſited in cooling; it had a taſte rather inſipid than ſweet.

It reddened the tincture of turnſol.

B. Lime water, and water of barytes produced in this liquor an abundant precipitate, partially ſoluble by nitric and muriatic acids.

C. Ammonia cauſed a leſs degree of precipitation.

D. Oxigenated muriatic acid ſeparated with flakes from the liquor.

E. Cauſtic potaſh, either ſolid or liquid, acted upon gizzard in the ſame manner as upon muſcular fleſh.

When ground together, ammonia was diſengaged from the gizzard; it became ſoft, of a reddiſh colour, and ſoluble in water. If this liquor be evaporated, it will depoſit fibres in cooling. Alcohol, by deſtroying the potaſh, ſeparated a flaky ſubſtance, ſoluble in water.

This aqueous ſolution gave a precipitate on the addition of lime-water, or muriate of lime or of barytes, as well as of ſome acids. The precipitate obtained by lime-water may be rediſſolved by the addition of more water, which proves that the mixture had not become truly ſaponaceous, but that the potaſh had merely diſſolved the animal matter.

F. The action of certain metallic ſolutions on the liquor of freſh gizzard was more or leſs perceptible, according to the facility with which the metal communicated its oxigen to the animal matter.

Nitrates of mercury and of ſilver, for example, were decompoſed, but the precipitates obtained by the action of theſe ſalts upon gelatine and the extract, quickly turned black, particularly that of mercury, and they were no longer ſoluble in nitric acid. The oxides had, therefore communicated a part of their oxigen to the gelatine and the extractive matter, which were thus united to the mercury, now approaching a metalline ſtate.

Oxigenated muriate of mercury was not decompoſed in this manner. The circumſtances, in fact, were no longer alike: the exceſs of oxigen which it contains ſufficing to oxigenate the two ſubſtances. Here the precipitate was very little coloured, and the metallic ſalt was only reſtored to the ſtate of mild mercurial muriate.

Some other metallic ſolutions produced in the liquor of freſh gizzard only gelatinous flakes; ſuch are the acetate of lead, and the ſulphate of copper and iron.

G. Aqueous tincture of nutgall changed the liquor into a kind of jelly.

Experiments on dried gizzard.

I have thought theſe experiments ſufficient for demonſtrating the nature of thoſe ſubſtances which were capable of ſolution in water; yet as gizzard is not adminiſtered in its freſh ſtate, but undergoes a proceſs which might cauſe a variation in the foregoing reſults, I again examined it in this latter point of view.

In drying the gizzard, I followed the preſcription already cited of M. Pia, and obtained a ſubſtance exactly anſwering his deſcription.

A. Reduced to powder, its taſte was inſipid, yet partaking ſtrongly of an animal flavour; its colour was a whitiſh grey.

Experiments on dried gizzard.

B. The aqueous decoction took a light yellow tint, and ſmelled like chicken broth.

It reddened the tincture of turnſol.

C. Lime-water and water of barytes cauſed the ſame kind of precipitate as in the decoction of freſh gizzard.

D. Oxalate of ammonia proved the preſence of lime.

E. Oxigenated muriatic acid ſeparated white flakes.

F. Nitric acid had a violent effect upon the dry gizzard; at a mild temperature it diſſolved it completely.

Nitric acid at eighteen degrees excited a ſlight efferveſcence, and by gradually increaſing its temperature, a ſeparation was perceived of azotic gas, then of nitrous gas, and of carbonic acid gas.

The liquor left in the retort was evaporated, in the expectation of obtaining cryſtals; but on cooling, none appeared. The evaporation was then continued, the reſult of which was a yellowiſh glutinous matter, tenacious, and of an exceſſively bitter and acrid taſte.

Water imbibed the acid, and preſented all the characters of the decoction of apples.

G. Metallic ſolutions preſented nothing particular, as in the experiments upon freſh gizzard, except that antimonial tartrite of potaſh was decompoſed, forming in the decoction a white precipitate.

H. Aqueous infuſion of nut-gall produced a leſs copious precipitate in this experiment, than it had with that upon freſh gizzard.

I. Dry and friable gizzard was digeſted in alcohol; but the liquor was ſcarcely coloured, even with the aſſiſtance of caloric.

This alcoholic tincture reddened that of turnſol, and gave precipitates with lime-water and water of barytes, as alſo with nitrate of ſilver; a proof that the alcohol has diſſolved only the ſaline particles.

L. The incineration of gizzard left a reſidue of a ſaline and alcaline taſte. Paper tinged by curcuma became of a deep brown.

This reſidue was partly ſoluble in water. The liquor contained ſulphate, muriate, and carbonate of potaſh.

The part not ſoluble, on being ſubmitted to the action of muriatic acid, diſcovered carbonate of lime, phoſphate of lime, and a ſmall portion of iron.

Hence it reſults, that the greater part of the ſalts contained in gizzard, is the acid phoſphate of lime; the preſence of muriate and ſulphate of potaſh is alſo obſervable.

Theſe ſalts are not only united with gelatine, but alſo with a ſmall quantity of extractive matter. It ſhould ſeem that the latter ſubſtance, and perhaps the gelatine, is oxigenated by the deſiccation of the gizzard; for in this ſtate they are leſs ſoluble in water.

Wiſhing to aſcertain the difference between pure gelatine, and that which had been oxigenated, I made experiments upon the former, of which the following is the reſult.

Experiments on gelatin,

Pure gelatine acquires different properties, according to the means employed in its oxigenation.

with metallic oxides.

Of the metallic oxides, ſome freely communicate their oxigen to gelatine, as the oxide of red-lead, and the red oxide of mercury; but the gelatine was combined with a part of the oxide, and could not again be ſeparated completely from it. In treating gelatine with the red oxide of mercury, a part of the oxide was reſtored to its metallic ſtate, and the remainder aſſumed a reddiſh brown colour.

And other means of oxygenation.

Superoxigenated muriate of potaſh heated with gelatine, cauſed no alteration in its nature.

Oxigen gas combined with it but ſlowly, and in ſmall quantity. After being for a conſiderable time ſubmitted to the action of this gas, the gelatine only ſuffered a change of colour; it became whitiſh, but its characteriſtics are ſtill the ſame.

Oxigenated muriatic acid preſented the following phenomena.

On pouring oxigenated muriatic acid gas into diſſolved gelatine, a whitiſh thick ſcum appeared on the ſurface, of a moderate thickneſs, the under ſide of which gradually changed colour, and became milky. The white filaments which ſwam in the liquor, together with the ſcum which floated on the ſurface, were ſeparated by filtering, and waſhed in cold and warm water till the water ceaſed to redden tincture of turnſol. The ſubſtance thus prepared preſented the following characteriſtics:

Properties of oxigenated gelatin.

1. It was capable of extenſion equally with gluten, and was of a white colour.

2. It was very light, and ſwam upon water.

3. When well waſhed, it retained little or no flavour,

4. Left expoſed to the air, it dried, and fell to duſt.

5. It did not redden the tincture of turnſol.

6. It was ſcarcely at all ſoluble in warm water. On boiling it a length of time, in a ſufficient quantity of water, it was reduced to an infinite number of particles, ſo minute as to be hardly perceptible; but as the heat was lowered, they re-united in a maſs as before the boiling.

7. Heated nitric and acetic acids diſſolved this ſubſtance; but it was precipitated in its original form, by refrigeration.

8. Trituration with cauſtic potaſh produced a ſeparation of ammoniac.

It differs from albumen.

This matter, it will be perceived, is neither gelatine nor albumen, ſince its properties are wholly different.

The gelatin of gizzard is probably oxigenated.

It appears probable, that the gelatine in gizzard acquires by drying, properties analogous to thoſe above deſcribed; which, with the changes obſerved in the extractive matter already mentioned, wonld certainly render dried gizzard leſs ſoluble in water.

We have no means of aſcertaining, for want of a proper object of compariſon, whether this difference be eſſential to the efficacy of gizzard; and I know not if freſh gizzard has ever been adopted in medical practice. I could only wiſh to aſcertain if its febrifuge quality exiſt in the oxigenated gelatine, in the extractive matter, or in the acid ſalt. Indeed, on comparing the quantity of gelatine adminiſtered to patients, according to M. Seguin, with the doſe of powdered gizzard, above-deſcribed, a great difference will be obſerved; and yet according to thoſe who have made uſe of it, a ſmall doſe of powdered gizzard is ſufficient to check the fever.

The compariſon which I have made of gelatine with gizzard is ſufficient to eſtabliſh a material diſtinction between them.

Experiments on gelatine.

Pure gelatine poſſeſſes a weak inſipid flavour; does not redden tincture of turnſol; is mucous and gluey between the fingers; aſſumes in the fire a concrete, ſolid, and tranſparemt appearance; and is ſoluble in boiling water.

Solution of barytes or of lime mixed with that of gelatine, cauſes a precipitation of phoſphate of lime.

Sulphates

Experiments on gelatine.

Sulphates of copper or tin, and acetate of lead, experience no decompoſition.

Nitrates of mercury and ſilver are decompoſed, but the precipitates are much leſs copious than thoſe produced with the decoction of gizzards

Solution of tartrite of antimony only thickened the liquor.

Alcohol likewiſe has but little power over gelatine. The precipitates obtained by means of the water of lime or of barytes, as well as that by nitrate of ſilver, are ſcarcely perceptible.

The decoction of freſh gizzard when ſuitably evaporated, leaves a coloured gelatinous matter, ſoluble in water, which reddens tincture of turnſol; gives copious precipitates with lime-water and water of barytes; decompoſes ſulphates of iron and copper, acetate of lead, muriate of tin, tartrite of antimonial potaſh, and nitrates of mercury and ſilver; the precipitates reſulting from theſe decompoſitions are generally too conſiderable to be attributed ſolely to the gelatine.

Dried and powdered gizzard poſſeſſes characteriſtics ſtill more diſtinct from thoſe of pure gelatine, whence I conclude that the latter ſubſtance has a different operation.

I leave practitioners to decide on the advantages which the medical art may derive from gizzard; it is for them to decide whether much confidence is to be placed in the notice inſerted in the Journal d'Economique. And if it ſhall appear that the medical uſe of this material has been attended with ſucceſs; it will perhaps be proper to attend particularly to other ſubſtances which have not hitherto been ſuppoſed to poſſeſs any febrifuge virtue; ſuch as the ſalts with exceſs of acid, the oxigenated extractive and even oxigenated gelatine.

VI.

On Pirite found in France by M. Cocq, Commiſſary of Gunpowder and Saltpetre Works at Clermont-Ferrant, with an Analyſis of this Subſtance. By J. J. DRAPPIER, *Teacher of Chemiſtry at the Polytechnic School.*[*]

Cryſtals of pirite found in the diſtrict of Puy de Dome,

M. COCQ found the cryſtals of pirite, in a porous grey porphyry, with a baſe of feldſpath, and containing cryſtals of quartz, forming a part of that chain of primitive mountains which ſupport the volcanoes of the diſtrict of Puy-de-Dome. Theſe cryſtals of pirite ſeparate from the rock, and leave in the porphyry an impreſſion perfectly ſmooth.

—and on the way to Menat.

He alſo found at the village of St. Avit, and in the vicinity of Pont-Gibaud, a ſubſtance which appeared to be pirite; in both ſituations it was ſo indeterminate as to render it impoſſible to pronounce exactly on its nature. But in returning to Menat, at twelve leagues to the north of Clermont, he perceived the granites reſuming the ſame appearance of thoſe which he had obſerved near Saint Avit and Pont-Gibaud, ſometimes the colour of the feldſpath inclined to purple, and oftentimes this ſubſtance appearing alone in the maſs of the granite, exhibited a beautiful purple.

The grey porous granite appeared again at intervals, with the appearance of the cryſtals obſerved in the ſame rock near St. Avit and Pont-Gibaud; at laſt, after a great many ſearches, he found the pirite well defined, and aſſuming a character much more determinate than that of Scheenberg.

Its Phyſical Characteriſtics.

Each cryſtal is a priſm of twelve faces, of a blackiſh or greeniſh brown, with a ſmooth ſurface.

Its colour is a greeniſh or blackiſh brown. Its form is a regular hexhedral priſm, of which all the lateral edges are truncated, which conſtitutes it a priſm of twelve faces. Sometimes the priſm has alſo a ſmall face at each of the angles of its baſe, which has not hitherto been remarked in the pirite of Saxony.

The ſurface of the cryſtals is ſmooth, and a little brilliant: in its interior, the pirite is dull, containing at times ſome particles of mica.

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[*] Journal des Mines, Vol. XVII. p. 307.

Its fracture is unequal, with a fine grain, approaching to a ſplintery fracture.

Its fracture is unequal, with a fine grain.

It admits of being ſcraped by a knife, and yields a duſt of a bright grey colour; it is tender, and does not adhere to the tongue, though it is a little unctuous to the touch.

Yields to the knife, does not adhere to the tongue, and is a little unctuous to the touch.

Beſides the ſize of its cryſtals, their faces, the ſubſtances to which they are found attached, added to the characters deſcribed, eſtabliſh the identity of this mineral with the pirite of Saxony.

The cryſtals found in Auvergne are more perfect than thoſe of Scheenberg; they exhibit no alteration, and the purity of their form removes all doubt of there being any neceſſity to claſs this ſubſtance as a new ſpecies.

The external characters of theſe cryſtals leave no doubt of their identity with the pirite of Saxony.

Analyſis by M. Drappier.

The pirite of France, ſeparated carefully from its bed, and reduced to a fine powder, is attacked and diſcoloured by muriatic acid. This acid diſſolves the oxide of iron, the colouring principle, and a portion of the alumine: but as it leaves a conſiderable reſidue, on which it appears to have no action, M. Drappier thought the method of analyſis ſhould be changed: he then took 100 parts of this ſubſtance, and kept it at a red heat in a crucible of platina for half an hour; after it was cooled, there was a loſs of ſeven parts. The remaining 93 parts were heated in the crucible for three quarters of an hour, with three times their weight of cauſtic potaſh, purified by alcohol. The fuſed maſs, detached from the crucible by diſtilled water, diſſolved entirely in muriatic acid. The ſolution evaporated almoſt to dryneſs, and then diluted with a freſh quantity of water, let fall a white precipitate, having all the characters of ſilex. This precipitate waſhed carefully and well dried, formed 0,46 of the ſubſtance ſubmitted to experiment.

Analyſis of the French pirite.

One hundred parts loſe 7 by heating.

The remainder fuſed with potaſh diſſolves in muriatic acid.

The ſolution evaporated and treated with water depoſits 46 parts precipitate.

The remainder of the muriatic ſolution was decompoſed by cauſtic potaſh. It immediately formed a precipitate, which ſoon diſſolved again in the exceſs of alkali, with the exception of 2½ parts of oxide of iron.

The reſidue treated with cauſtic potaſh leaves 2½ parts iron oxide.

The alkaline ſolution ſaturated by an acid, depoſited 42 parts of an earth, which had all the properties of alumine. All theſe precipitates, before they were weighed, were waſhed carefully, and heated to redneſs in a crucible of platina.

The alkaline ſolution ſaturated by an acid depoſits 42 parts alumine.

Analyſis of the Pirites of France compared with that of Saxony

Analyſis tabulated.

	Pirite of France.	Pirite of Saxony analyſed by Klaproth.
Silex - - - -	46,00	29,50
Alumine - - -	42,00	63,75
Oxide of iron -	2,50	6,75
Loſs by calcination	7,00	
Loſs - - - -	2,50	
	100,00	100,00

Remarks on the difference of the pirite of France from that of Saxony.

M. Drappier thinks, on comparing his analyſis with that made by M. Klaproth, that it may be concluded, ſuppoſing there was no error in either analyſis, that either the pirite of France is not the ſame ſubſtance as that of Saxony, or that minerals having the ſame external characters, and eſpecially the ſame form, may vary both in their chemical properties, and in the proportions of their conſtituent principles. M. Klaproth ſays that acids have no action on the pirite of Saxony, that he found much difficulty in operating on it by potaſh, and that, in order to ſeparate its parts, he was obliged to treat it twice with this alkali. The ſame chemiſt appears not to have found any water in this ſubſtance. This difference, it is true, may be explained, if it is conſidered that the pirite of Saxony contains more alumine, and that it adheres to the tongue, while that of France has not this property, probably on account of the water which it contains.

VII.

Experiments, ſhewing, contrary to the Aſſertions of Morichini, that the Enamel of Teeth does not contain Fluoric Acid. In a Letter from WM. BRANDE, *Eſq.*

To Mr. NICHOLSON.

SIR,

Gay Luſſac on fluoric acid in animal ſubſtances.

HAVING ſeen in one of the laſt numbers of the *Annales de Chimie*, an article entitled, "Lettre de Monſieur Gay-Luſſac a Monſieur Berthollet, ſur le preſence de l'acide fluorique dans les ſubſtances animales," &c. I was ſurpriſed to find that a chemiſt at Rome, of the name of Morichini, had diſcovered fluoric acid united to lime in the enamel of human teeth. The extraordinary reſults of theſe reſearches, induced me to repeat them; but before I mention the experiments from which I have drawn concluſions different from thoſe of the above-mentioned chemiſt, it may perhaps be proper to quote that part of Gay-Luſſac's letter which relates to the preſent ſubject:

—and ſaid by Morichini to exiſt in the enamel of teeth.

“ M. Morichini having detached ſome of the enamel from human teeth, ſuppoſed that it might bear ſome reſemblance in its compoſition to the enamel of the foſſil teeth of an elephant, in which, on a former occaſion, he had detached fluoric acid; he therefore ſubjected it to analyſis, and perceived, to his great ſatisfaction, that it contained a large proportion of fluoric acid.

Quotation to that effect.

To render theſe experiments more concluſive, he ſubmitted portions of the two ſpecies of enamel, viz. that of the foſſil, and human teeth, and likewiſe fluat of lime, to the action of ſulphuric acid, and found that the laſt of theſe three ſubſtances yielded fluoric acid in the greateſt abundance, and that the enamel of foſſil teeth yielded ſomewhat more than that of human teeth; but Morichini remarks, that this difference is merely owing to the preſence of animal matter in the two kinds of enamel, and that the diſengagement of the acid from the fluat may be retarded, by adding a little gelatine to that ſubſtance, after it has been calcined, and then drying the compound. He moreover obſerves that the vapours which ſulphuric acid diſengaged from any of theſe three ſubſtances, had the property of acting on glaſs, of depoſiting a ſiliceous film on water, and other properties, which it is ſcarcely neceſſary to mention.

Morichini ſays that the enamel of recent teeth afforded fluoric acid as well as that of foſſil teeth;

and that the ſulphuric acid diſengaged vapours that corrode glaſs, &c.

According to Morichini's experiments, one hundred parts of the enamel of human teeth contain 30 parts of animal ſubſtance, and 22 parts of fluat and phoſphate of lime, with ſome magneſia, alumine, and carbonic acid. He has not yet been able to ſeparate the fluoric and phoſporic acids from each other, but thinks that the proportion of the latter muſt be extremely minute. M. Morichini has alſo obſerved that the enamel of the foſſil teeth of the elephant differs from that of human teeth, in containing a ſmaller proportion of animal ſubſtance and phoſphoric acid; but he thinks that the phoſphoric acid which he found in the enamel of human teeth may have been derived

Component parts of enamel of teeth according to Morichini.

from a portion of the bony part from which the enamel is ſeparated with great difficulty. But the moſt intereſting and unexpected reſult is, that fluoric acid exiſts in animal ſubſtances: a diſcovery of the greateſt importance. Theſe experiments oppoſe the preſent opinion concerning the compoſition of enamel, for Mr. Hatchett in his analyſis of this ſubſtance has only detected phoſphate of lime.

Morichini profeſſes to have proved my facts by repeated experiments.

The reſult of Mr. Hatchett's experiments, together with thoſe which were ſubſequently publiſhed by Mr. Joſſe, in the Annales de Chimie, Tom XLIII. rendered it neceſſary for M. Morichini to ſubmit his opinion to accurate inveſtigation, and after having made a numerous ſeries of experiments on the ſubject, he obſerves, that he cannot entertain a doubt, that the enamel of human teeth conſiſts chiefly of fluat of lime.

General remarks by Gay-Luſſac.

After ſome obſervations on the compoſition of ivory, M. Gay-Luſſac concludes this part of his letter, by obſerving that there is an immenſe field laid open in that part of chemiſtry which relates to animal ſubſtances, if it were merely to ſearch for fluoric acid. Morichini has undertaken an inveſtigation of the ſubject: but ſo much remains to be done, that the exertions of many chemiſts will be requiſite."

The author's experiments ſhew the contrary.

I ſhall now relate ſome experiments, which will ſhew that fluoric acid does not exiſt in the enamel of human teeth, but that this ſubſtance conſiſts chiefly of phoſphate of lime, as originally ſtated by Mr. Hatchett.*

Enamel of human teeth was ignited, pulveriſed, and ſubjected to ſulphuric acid. The fumes did not corrode glaſs.

One hundred grains of the enamel of human teeth, detached from what is uſually termed the bony part, but which appears to conſiſt of a ſubſtance of the nature of ivory, were kept for a few minutes in a red heat, and then pulveriſed. The enamel, thus reduced to powder, was put into a platina crucible, in which a piece of a glaſs rod was placed horizontally in ſuch a manner as to be about an inch and a half above the enamel. Half an ounce of ſulphuric acid was then added, and the crucible being covered with a clean plate of glaſs, the heat of a lamp was applied, and diſtillation carried on for half an hour. During the proceſs, white ſuffocating fumes were extricated; but on removing the glaſs which cloſed the top of the crucible, neither this, nor the rod below it were in the leaſt acted upon; which certainly would have happened, had any fluoric acid been preſent. Finding this, therefore, to be the caſe, I proceeded as follows:—Fifty grains of the ſame enamel were introduced into a ſmall glaſs retort, and a little ſulphuric acid being added, diſtillation was carried on nearly to dryneſs, but in ſuch a manner, that the gaſeous products might be received over mercury. A ſmall quantity of ſulphuric acid gas was diſengaged, and what remained in the retort, conſiſted, as far as I could aſcertain, of a mixture of ſulphate of lime, phoſphoric acid, and a ſmall portion of ſulphur, ariſing from a decompoſition of a ſmall part of the ſulphuric acid by the animal matter, exiſting in the enamel.

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* Vide Phil. Tranſ. 1799, p. 328.

neither was there any extrication of fluoric acid by adding ſulph. acid to enamel and diſtilling over mercury.

I have the honour to be,

Sir,

Your moſt obedient ſervant,

WILLIAM BRANDE.

Arlington Street,
Feb. 15, 1806.

VIII.

A Memoir on taking the Levels of the whole Surface of France By P. S. GIRARD, *Chief Engineer of Bridges and Highways, &c.**

IF the ſurface of the earth were formed by the revolution of a curve round its axis, it would be ſufficient, in order to determine the reſpective poſitions of different points upon it, to meaſure their diſtances from the interſection of that ſurface made by the plane of the equator and any particular or aſſumed meridian.

The method by which geographers determine the poſition of a place.

Thus geographers, conſidering the earth as perfectly ſpherical, have determined the poſition of any given place by the conjunction of two co-ordinates, one of which is the arc of the meridian, compriſed between the place and the equator, and the other an arc of the circle parallel to the equator, compriſed between the place and any aſſigned meridian.

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* Journal des Mines, Vol. XVII. p. 297.

As theſe two co-ordinates interſect each other at right angles, it is apparent that the method of geographers, for determining the poſition of any place on the earth, is the ſame as that by which the poſition of a point on a plane is commonly determined.

is not exact, on account of the inequality of the earth's ſurface.

But this proceſs, which would completely anſwer the views of geographers, if the terreſtrial ſphere were regular, ceaſes to be exact when the irregularities and protuberances are conſidered, with which the ſurface of this ſpheroid is covered.

This true poſition of a place is in a line perpendicular to that aſſigned by geographers.

The poſition of any place depends in reality, according to this hypotheſis, on a third co-ordinate, which is ſuppoſed to be drawn perpendicular to the point of interſection of the two others.

This third co-ordinate ought to be taken vertically over the place of which the poſition is to be determined, and its meaſure reckoned from the place itſelf to its arrival at an imaginary ſurface, produced by the revolution of a known curve round the axis of the earth.

The level of the ſea affords a ſpherical ſurface, from whence to meaſure thoſe perpendiculars.

But it is known, that if our globe were ſurrounded by a fluid maſs, all other force being ſuppoſed to be abſent but that of terreſtrial gravity, the ſurface of this fluid maſs would be that of a ſpherical ſolid, of which the mean ſurface of the ſea, in its actual ſtate, repreſents a part: It appears then convenient to chooſe, for the third co-ordinate here mentioned, that portion of a vertical line paſſing through any place, which is compriſed between that place and the mean ſurface of the ſea, ſuppoſed to penetrate the globe and to be extended beneath the continent.

This is the beſt, though not the only method:

But it is the moſt ſimple, and is beſides generally adopted.

The true poſition of a place determined by annexing its

We have ſaid that the choice of this line would be convenient; becauſe, in reality, the poſition of a point on the terreſtrial ſurface may be determined by adopting any other ſyſtem of co-ordinates; for example, by fixing the poſition of this point, by three planes mutually interſecting at right angles; but, beſides the advantage of greater ſimplicity in the expreſſion of the circular co-ordinates, they have moreover, that of being generally adopted; for the geographical charts, hitherto prepared, may be conſidered as the projection of the continents and iſlands on the mean ſurface of the ſea; ſo that there only remains, in order to render geography perfect, to add to the latitude and longitude of all the places on the earth, the vertical height which they are elevated above the ſurface of the ocean.

vertical elevation above the ſea to its latitude and longitude.

The object of this memoir is to indicate the means of determining this vertical heighth, by their particular application to the territory of France.

Management proper for aſcertaining theſe poſitions ſhewn in the inſtance of France.

It is evident, that all the operations neceſſary for this determination, may be reduced to a ſeries of levels made in determinate directions.

Nature itſelf has pointed out theſe directions, by the lines of *greateſt declivity*, which the large rivers, and thoſe which flow into them, form on the ſurface of the earth.

Thus, France being divided into five principal baſons, by the Rhine, the Seine, the Loire, the Gironde, and the Rhone, —the levels of the courſe of theſe rivers, from their ſources, or from their entrance into France, to their terminations in the ocean, would form the firſt baſis of the work propoſed to be undertaken.

The levels of the five great rivers of France would form the baſis of this operation for that country.

After having aſcertained this firſt baſis of the general operation, the levels of the ſtreams by which the great rivers are ſupplied, ſhould be next taken, and theſe ſtreams ſhould be conſidered without any regard to thoſe of the third order, by which they are themſelves maintained.

The levels of the ſtreams which ſupply thoſe great rivers ſhould be next taken.

At the ſame time, the levels of the rivers of the ſecond rank, which fall into the two ſeas, ſhould be taken; ſuch as the Eſcaut, the Somme, the Orne, the Vilaine, the Charente, the Adour, the Herault, &c.

The levels of the rivers of the ſecond rank taken at the ſame time as thoſe of the firſt.

The declivities of the beds of the ſecondary rivers being known, thoſe of the rivers of the third, fourth, and fifth orders, &c. ſhould be determined ſucceſſively, according to ſpecial inſtructions which ſhould be given for this purpoſe.

The levels of rivers of the third, fourth, and fifth order, taken.

By thus claſſing the operations relative to the general levels of France, and by arranging their reſults in order, as they were obtained, all the data would be ſoon collected, which were neceſſary for tracing the elevation of its territory on a geographical chart already prepared.

This tracing of the elevations would be effected, by joining all the points on one level by the ſame line.

Elevation of the ſurface of France to be expreſſed in the rough, by joining all the points on one level by the ſame line.

Theſe *lines of levels* might be ſuppoſed to be elevated perpendicularly, one above the other, by a determinate ſpace, conformable to the ſcale of the chart on which they were traced.

It is evident, that theſe lines would repreſent the borders of the coaſts of the ſea, if it was ſuppoſed that its mean level ſhould be elevated ſucceſſively to the ſame heighths which they repreſented.

M. Triel prepared the ſketch of a map on this plan.

It was according to this idea that M. Dupain Triel prepared a phyſical chart, mentioned by M. Lacroix, member of the National Inſtitute, in his introduction to Pinkerton's Geography; a chart which, from the defect of materials neceſſary for its conſtruction, preſented only the ſketch of a work, the extent of which would require for its perfection an union of means, which could not be at the diſpoſal of any particular individual.*

The order has been pointed out in which this work ought to be executed, and we ſhall now examine how it ſhould be performed.

The courſes of the great rivers ſhould be divided into portions, and the levels of each taken by obſervers at the ſame time.

The bed of each of the great rivers muſt be divided into a certain number of portions, and each portion ſhould be levelled by obſervers, who ſhould operate at the ſame time.

Theſe obſervers ſhould place accounts of their operations at each extremity of the portions of the baſons with which they were charged; and as the levels of the ſecondary rivers ſhould be connected with thoſe of the principal rivers, it would be neceſſary alſo to place accounts of the operations at the mouth of each of the influent ſtreams.

The levels ſhould be taken on the banks of the rivers, without any regard to the ſurface of the water. If it were thought uſeful to determine the declivity of this ſurface, it would be eaſy to aſcertain it, by levels taken at the ſame time with the others, at certain diſtances from each other.

The reſults to be collected into a general ſyſtem.

When the different obſervers have completed their reſpective obſervations, the reſults muſt be collected, to form the ſeries of levels of one of the beds. And in the ſame manner the levels of all the reſt ſhould be obtained.

After this, a general ſyſtem ſhould be formed from thoſe particular levels, by connecting together the different beds, by operations directed from one to the other, according to thoſe lines which would afford the greateſt facility.

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* Compare this with Mr. Churchill's plan, at p. 224 of our XIth. Vol.—T.

There only remains to determine to what agents Government ſhould entruſt the performance of the general levelling of France, in order to have it executed with the greateſt exactneſs, ſpeed, and economy.

The engineers of bridges and highways would be the moſt proper perſons to employ in this work.

The engineers of bridges and highways, already placed in the different departments, where this operation ſhould be performed, are evidently the only perſons to whom it could be confided, ſo as to fulfil theſe three conditions.

In fact, the execution of all projects relative to the eſtabliſhment of communications by land or by water, require, that the elevation in relief of the country, through which the works ſhould be carried, ſhould be known. The theory and practice of levelling form an eſſential part of the inſtruction given to the engineers of bridges and highways; and greater reliance may be placed on the exactneſs of the reſults which they might furniſh, becauſe the uſe of the inſtruments neceſſary to this operation, is more familiar to them.

On the other hand, there are none of thoſe engineers who could not dedicate ſome days of the ſummer to taking the levels of that portion of ſuch great rivers, or ſtreams, as ſhall traverſe his diſtrict; and as it is eaſy to take the levels of four or five kilometres (about three Engliſh miles) each day, eſpecially when the line to be levelled is previouſly determined by the direction of the river or current of water, it is certain, that the engineers of the bridges and highways might collect, in a very ſhort period, very minutely detailed materials for a phyſical chart of France.

They could perform it without expence to the nation.

Laſtly, theſe materials would be collected by them with the leaſt poſſible expence, becauſe Government would neither have to ſupport the coſt of extraordinary journies, nor the purchaſe of inſtruments, as the engineers are already, by the very nature of their employments, diſperſed over the ſeveral diſtricts where it would be neceſſary to operate, and are, at the ſame time, provided with the different inſtruments required for this purpoſe.

It would tend much to their own benefit to have it effected.

It may alſo be added, that the taking the general levels of France appears to be, with the more propriety, a work that ſhould be performed by the engineers of bridges and highways, as they would be the firſt to profit by this operation in putting their projects into execution.

Suppoſe then that the engineers of bridges and highways were charged with the performance of this work, let us conſider how, after ſome years, the exactneſs of the reſults, which they had collected, could be ſufficiently aſcertained.

Let us take, for example, the bed of the Loire, whoſe courſe is of great extent.

The chief engineers of the departments of the Upper Loire, of the Loire, of the Saone and Loire, of the Nievre, of the Loiret, of the Loire and Cher, of the Indre and Loire, of the Mayenne and Loire, and of the Lower Loire, would be ordered to furniſh, during the year, the levels of that part of the courſe of the Loire which traverſed their reſpective departments.

According to the new organization of the ſervice of bridges and highways, theſe nine departments require twenty-two engineers, in the diſtrict of each of whom would be found a portion of the work to be performed.

The total extent of the Loire is about ninety muriameters (about 550 miles), which being divided among twenty-two obſervers, would give to each of them little more than forty kilometers (about twenty-five miles) of levels to execute.

Levels of the Loire might be taken by the 22 engineers in that diſtrict, in one ſeaſon.

There is reaſon to believe, from experience, that the twenty-two engineers employed on the courſe of the river, would finiſh, in leſs than one ſeaſon, the levels of the whole river.

The ſame thing may be affirmed of the engineers placed in the departments traverſed by the Rhine, the Seine, the Gironde, and the Rhone. It appears then beyond a doubt, that, at the end of the firſt year, the chief part of this phyſical chart could be completed, to which the farther details might be afterwards added.

Levels of France, when taken, might be afterwards verified by the newly appointed engineers.

Whatever care may be beſtowed in taking levels, their verification is always an uſeful operation. That of the general levels of France might be made as often, and in whatever circumſtances it ſhould be judged neceſſary. It would be ſufficient for this purpoſe, to direct the newly appointed engineers to repeat, in the departments to which they might be ſent, the obſervations of their predeceſſors; which, beſides the advantage of confirming or correcting the reſults already obtained, would give an opportunity to the new engineers of acquiring, in perſon, a knowledge of the elevation of their reſpective diſtricts in relief.

Facility of this work.

The facility and promptitude with which the engineers of bridges and highways might execute this work, will be apparent, if it be recollected that, at the time when the major part of the great roads in France were formed, and when a general ſyſtem of internal communications was deſired to be eſtabliſhed, M. de Trudaine, aſſiſted by M. Perronnet, cauſed plans to be taken of all the principal roads, from their commencement to the frontiers. There was joined to the plan of the road properly ſo called, that of the country bordering on it, to the diſtance of three or four hundred yards at each ſide; a work which evidently required more time than ſimply taking the levels of a determined line, ſuch as we propoſe; and yet the engineers of bridges and highways, or their pupils, employed in taking thoſe itinerary plans, completed from five to ſix leagues of them each month.

Geological operations already performed in France, urged as a motive to commence this work;

which would complete the natural geography of countries.

The general utility of the operation, of which a ſketch is here given, will ſooner or later determine ſome of the nations of Europe to undertake it. France, on whoſe territory has been lately executed ſome of the fineſt geological operations which were ever performed, and where, for the firſt time, a ſyſtem of univerſal meaſure has been eſtabliſhed on an invariable baſis, ſeems to be particularly called on, to give, on this occaſion, the firſt example of a work, which, by completing the natural geography of countries, will furniſh new facts to geology, and to thoſe different parts of natural hiſtory which depend on it.

IX.

Obſervations and Experiments on the Compoſition of Water, and other Elementary Doctrines. By H. B. K.

To Mr. NICHOLSON.

SIR,

As two papers have appeared in your Journal, both of which militate againſt the reſult of my experiment, and as Mr. Accum has been concerned in one of them, I therefore think it incumbent on me to anſwer them.

The gaſes obtained from water by galvaniſm, ſmelled of nitrous gas after exploſion, and gave nitre with potaſh.

I was glad to ſee my experiments in your Journal, as it has ſo extenſive a circulation. I ſhall now give you the analytical part to confirm my former experiments; as by them I had, I hope, given ſtrong evidence, that acids are neceſſary in forming gaſes. Having collected a great quantity of the gaſes produced by the galvanic pile, I introduced them into a ſtrong glaſs tube, cloſed at one end, the other end I afterwards cloſed, having previouſly introduced to the gaſes a ſmall quantity of a ſolution of potaſh: through this tube the electric ſpark was made to paſs, it having ſmall openings to admit the wires of communication. Upon their combuſtion, the ſmell of the nitrous acid vapour appeared, both from its colour and ſmell; and the tube being moved up and down, ſo as to allow the vapour and the ſolution of potaſh to come in contact; the ſolution being examined ſome ſhort time after, it gave evident and unequivocal ſigns of the nitrate of potaſh.

On Pacchioni and Riffant's experiments.

I ſee in your Journal, Mr. Riffant's experiments, in anſwer to Mr. Pacchiani's paper. Indeed, in reading Mr. P.'s experiments, nothing could appear more vague and wild than that water, by having oxigen, the ſuppoſed acidifying principle, taken from it, ſhould become a ſtrong mineral acid. Mr. Riffant's ſecond experiment directly contradicts my experiment, on the ſuppoſition that water is a compound body; but if examined upon my ſuppoſition, that the acids are neceſſary in forming the gaſes, and that the water is only neceſſary in forming the water of compoſition, I hope I ſhall be able to prove that his experiment confirms my opinion. There were very little of gaſes formed by this experiment, and the wires were very much calcined: now this calcination was from the acid, or acids, I proved by repeating the very ſame experiments; but only inſtead of diſtilled water, I uſed a ſolution of potaſh, and inſtead of the wires being calcined, they were not ſenſibly acted upon, and the potaſh became nitrated.—Now, Mr. Nicholſon, I (ſeriouſly and ardently) call upon your numerous readers to perform this experiment, which I think muſt be deciſive.

Againſt the doctrine of the compoſition of water: it is urged that the gaſes obtained in galvaniſm vary from different cauſes.

I can but ſmile at the French chemiſts, in making the proportion of the gaſes ſo exactly to tally with their opinion of the compoſition of water; but I have in my experiments found very different reſults; the kind of gaſes depending a good deal upon the wires uſed, the different metals, their length, and the different liquors between the plates of the pile; all of which had a ſenſible effect upon the gaſes, both upon the quantity produced, and their kind; the calcinable wires when long producing the moſt inflammable kinds, and the leſs calcinable metals the more of the oxigen kind, and the longer the wires the more in volume were the gaſes.

Remarks on Mr. Northmore's experiments.

Mr. Northmore, in your Journal, endeavours to prove the formation of the nitric acid from the compreſſion of gaſes.— Upon inveſtigation, his experiments will, I think, be found very vague and inconcluſive: that gaſes from active compreſſion will produce both heat and water, has been long known. The firſt experiment was in condenſing hidrogen, oxigen and nitrogen gaſes, two pints of each. He ſays they produced "white floating vapours, probably the gaſeous oxide;" but in experiment the ſeventh, he obſerves, "the hydrogen produced white clouds at firſt, *quære* ammonia." So without any chemical examination of theſe white clouds, they are at firſt ſuppoſed to be the gaſeous oxide, and afterwards ammonia, juſt according as his theory dictates to him. In the fifth experiment, he ſays, "and the reſult was only a ſmell of gaſeous oxide of nitrogen, a few yellowiſh fumes." Here then the gaſeous oxide produces a yellowiſh colour, though in the firſt experiment it was a white colour.

The acid produced was, from the ſame vague opinion, ſuppoſed to be the nitric; but this he endeavours to examine in the next experiment; firſt by a good teſt, in expoſing it to lime water; and he ſays, "Some yellow particles were ſeen floating upon the lime water; theſe particles probably aroſe from the reſinous ſubſtance uſed in faſtening on the cap of the receiver being diſſolved by the nitrous gas formed during condenſation. Here then was the lime water affected. I ſay with confidence, theſe flocculi in the lime water were from the carbonic acid produced, and why they appeared yellow was from their being ſeen through the gaſes, being clouded with an orange colour, which, as he obſerves, they put on when they were condenſed.

That acids are neceſſary in forming oxigen gas, I hope appears very clear from my experiments; therefore when it forms combuſtion with inflammable bodies, it is rational to ſuppoſe that an acid will appear upon its decompoſition. If the combuſtion is active, as in the French experiments in condenſing

oxigen

Remarks on Mr. Northmore's experiments.

oxigen and hidrogen gaſes, the heat produced is ſo active as to make an exploſion of the gaſes; but if a ſlow combuſtion, it will leave the oxigenized acid in a gaſeous ſtate, as carbonic acid gas, which, I ſuppoſe, was the caſe in Mr. Northmore's experiments. His next experiment of examining the acid: He compreſſed the gaſes upon two ſcruples of the ſolution of potaſh; he ſays, "there was ſcarce enough acidity to tinge the edge of the teſt paper; of courſe, I could not effect the formation of the nitrate of potaſh." But always to aſſign ſome reaſon for the failure, he ſays, "This quantity (of gaſes) was hardly ſufficient for the receiver's capacity;" but there was the ſame quantity in this experiment as in the others; nay, in the next experiment (the ſixth) there was identically the *ſame quantity and in the ſame proportions*; and in this fifth experiment, he found ſo little acid, as he ſays, "Scarce enough acidity to tinge the edge of the teſt paper; of courſe I could not effect the formation of the nitrate of potaſh." Now upon the ſuppoſition that the carbonic acid was formed, it would unite with the potaſh, and therefore the mixture would be leſs ſaturated with it: But if the acid was ſo ſtrong as he ſpeaks of in the ſixth experiment, from the very ſame proceſs, as he ſays, "Which moiſture was ſtrongly acid to the taſte, coloured litmus, and when very much diluted with water, acted upon ſilver." Now if Mr. Northmore will conſult the writings of chemiſts, (Dr. Black's lectures, for inſtance); in Vol. II. the doctor ſays, "that the nitric acid requires a little water to reduce it to the ſtrength of aqua fortis; in order to act upon ſilver, therefore, in this experiment, the acid muſt have been in the concentrated ſtate of the nitric acid, as it required water to be diluted to make it act upon ſilver; but probably Mr. N. does not know that water impregnated with hidrogen gas will colour ſilver; which I ſuppoſe to have been the caſe here.

This reaſoning muſt appear to be moſt extraordinary: this vaſt quantity of nitric acid produced was even to penetrate into the cap of the receiver; but very unfortunately for this ſuppoſition, chemiſts are of opinion that acids will not diſſolve reſins. Mr. Hatchett has promoted their operation upon each other by uſing the ſtrong nitric acid; but this was a difficult and tedious proceſs, not during the tranſitory action of a little time, by compreſſion; and where the reſin was ſo con-

cealed;

Remarks on Mr. Northmore's experiments.

cealed; ſo that the acid could not get to act upon it, being placed within the cap of the receiver; therefore the ſmall quantity of weak acid formed in Mr. N.'s experiment could not rationally be ſuppoſed to have penetrated to it, even if it was in a high concentrated ſtate; but it muſt have been much diluted with water, as there was water alſo produced in this experiment: Alſo, if it was in this high concentrated ſtate, and in that abundance as to enter into all the crevices, it would eaſily have been detected, and his fifth experiment was for this purpoſe, but it failed; he could find no nitrous acid.

In experiment the ſeventh, he ſuppoſed he had formed ammonia, and he ſays in this very experiment, "Some vapour was generated, which was, as uſual, ſtrongly acid." How comes it that this acid, which was ſuppoſed to find out the reſin, ſo perfectly concealed, could not find out the ammonia, which was formed along with it in the proceſs, and ſo univerſally diſperſed as to form white clouds.

The third experiment: "Two pints of carbonic acid and two of hydrogen was ſubjected to condenſation. The reſult was a watery vapour, and a gas of rather offenſive ſmell." This compreſſed gas I found to be ſimilar to Mr. Cruikſhanks's gaſeous oxide of carbon from the acid air and the phlogiſtic air ſaturating each other.

Mr. Northmore apologiſes for giving theſe experiments "until he had brought them to a greater degree of perfection," but at the concluſion he alſo ſays, "Beſides the above, I have made various other experiments with different gaſes, &c." But as he ſays nothing more of theſe *imperfect experiments*, there are no hopes of his correcting them; he appears to have exhauſted his reſearch, and we have ſeen with what ſucceſs. There appears ſuch an ardent deſire to ſupport the Lavoiſierian theory: but if it has always failed from the experiments of Lavoiſier himſelf, Mr. Cavendiſh and others, I am afraid we have little to expect from theſe new ſupporters. I might make many other obſervations, but theſe will, I preſume, be thought enough.

Experiments of compreſſion of the gaſes.

It will be expected in contradicting Mr. N.'s experiments I ſhould make ſome of my own; I muſt own my apparatus was not ſo good as his; yet I hope ſufficient to prove my opinions. I had the barrel of a large blunderbuſs, and ſtopped its priming hole, and having filled it either with ſand or diſtilled water, I then tied to its mouth a bladder filled with the different gaſes I wanted to compreſs. Upon pouring out the ſand or water into the bladder, the gaſes entered the barrel, and then having a ſtrong iron ram-rod made perfectly air-tight, it was forced down upon the gaſes by a long iron lever, by which means I was capable of making a ſtronger condenſation than reducing them to one fifth of the volume. The reſults of theſe experiments were, that all the different gaſes, by being compreſſed, gave out heat and moiſture: The hydrogen gas, the greateſt proportion of moiſture to its ſpecific gravity. That when oxigen and hydrogen gaſes were compreſſed, there was an acid which produced flocculi in lime water; and that nitrogen gas was not neceſſary to the production of the acid, but rather retarded its production. The nitrogen gas obtained by the nitric acid and animal ſubſtances ought not to be uſed in theſe experiments, as it is partly acid of itſelf; but the nitrogen of the atmoſphere ought to be made uſe of, being previouſly paſſed through lime water.

They gave out heat and moiſture.

Oxigen and hydrogen gave acid.

Mr. Nicholſon, I have condenſed this communication as much as poſſible, in order that it might not occupy too much room in your Journal. I am

Your's, &c.

H. B. K.

London, February, 15, 1806.

X.

On the Conſtruction of the Sails of Ships and Veſſels. By MALCOLM COWAN, *Eſq. Captain in the Royal Navy.*[*]

IT appears from the conſtruction of the ſails of ſhips and veſſels, belonging to every nation, that it is a ſubject no one has hitherto taken much pains to inveſtigate; but the maritime world have been content to uſe them, as they found them, every one following the beaten track of his predeceſſors, without examination.

The ſails of ſhips have been long without improvement.

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[*] Extracted from an eſſay, by the author, who has letters patent for the ſails.

That the ſails of ſhips have been hitherto ſo conſtructed by all European nations, ſo as to be only managed with great labour and danger; and that when managed with the greateſt ſkill, they are very far from being of that utility which they ought to poſſeſs, and are capable of having, is inconteſtible.

They are worked with difficulty and danger.

Ships are driven on ſhore every winter, which might, with proper ſails, have eſcaped every danger. The loſs of one ſail, in many ſituations, is followed by the inevitable loſs of the ſhip and crew. Sails are often ſplit in hauling up to reef, and it may be neceſſary to reef a ſail that is worn, to preſerve it from ſplitting; hence the neceſſity of the ſails being conſtructed to reef without ſtarting tack or ſheet.

Many ſhips have been loſt by not having time, or drift, to haul their courſes up, to reef them on the yard, by which they riſk their ſplitting; a circumſtance which alone muſt convince the ſeamen of the utility of having ſails that can be reefed without taking their effect off the ſhip.

Many dangers may be avoided, by carrying ſail with ſafety to the maſts and yards. A ſhip can carry top gallant ſails that reef at the foot, with ſafety, when other ſhips muſt furl theirs; an evident advantage in many ſituations.

Advantages of ſails which are reefed at the foot inſtead of the head.

The top ſails of ſhips, with one or two reefs at the foot, can be reefed in a minute by one ſeaman at each lower yard arm, while they remain ſet with the top gallant ſails over them, by only ſettling the hallyards; by which a ſhip in ſqually weather, on many occaſions, would have a great advantage, particularly in chace, &c. or when caught by a ſudden ſhift of wind on a lee ſhore, or obliged to haul ſuddenly to the wind from ſailing large.

The facility with which ſails that reef at the foot, can at all times be managed, would enable ſhips to make quicker voyages, and prevent them often, when weakly manned, from detaining fleets; by the difficulty and danger of carrying ſail, being entirely removed, muſt enable merchant ſhips to be navigated with fewer hands, which would be a conſiderable ſaving of expence, and a great advantage in time of war in particular, when men are ſo ſcarce.

The ſeams ought to be horizontal.

If the ſails were made with horizontal cloths and ſeams, the ſails would ſtand better, particularly in a gale of wind; as the ſtrongeſt direction of the cloths and ſeams would be oppoſed to the greateſt force of the wind, which acts horizontally;

tally; and ſhould the ſail ſplit in that direction, it would ſtill remain full, and be leſs liable to blow away altogether, which is generally the caſe when a ſail ſplits in a vertical direction. Storm ſtay ſails ſet purpoſely with the cloths horizontal, have proved this beyond a doubt.

Many ſeamen are loſt every winter, by falling overboard from the yards while reefing the ſails, as it is more dangerous and requires longer time to perform in a gale of wind, than furling the ſails, which is not ſo often neceſſary as reefing.

Other advantages from improvements in ſails, &c.

Ships may ſometimes avoid a lee ſhore, by carrying a timely preſs of ſail, and when in that perilous ſituation, in a gale of wind, the ſafety of the ſhip may ſolely depend on the ſails being kept ſet; though it may be neceſſary to reduce them, either to ſave them, or eaſe the ſhip. The common ſails require to be hauled up, to be reefed, at the riſk of ſplitting them, at a time perhaps, when the ſhip is in imminent danger, from the want of ſea room; and the beſt ſeamen of the crew muſt be ſent on the yards when they poſſibly may be much wanted on deck.

Whole fleets are often caught by a ſudden ſhift of wind, of a lee ſhore, thrown into confuſion, and obliged immediately to reef their ſails, at the ſame time the ſhips may require the whole of their crews on deck, to attend the working of the ſhip, to keep clear of each other; particularly when it happens in the night time, with the wind ſqually and variable.

When ſhips from foreign voyages, enter the Engliſh or Iriſh channels, in the winter time, when the days are ſhort, and the nights long, with weak or diſabled crews, or men not accuſtomed to cold or froſt, ſuch as Laſcars, Negroes, &c. it is with the greateſt difficulty they can be prevailed on to go aloft; but ſhould they get on a lee ſhore, which all ſhips are liable to, and with a helpleſs crew, nothing can exceed the horror of their ſituation, ſhould they not be able to proportion their ſail to the wind in time to ſave the ſhip.

Naval improvements are of great importance to the ſtate.

To facilitate the working of ſhips, by the moſt approved means, is an object of greater conſequence to a maritime nation than many are aware of, even in a commercial point of view. The little alteration that has been made in ſhipping for many years paſt, ſhews with what indifference attempts at improvements have been regarded; many of which have been tried, proved, and neglected, while others have failed from the unavoidable expence, neceſſarily attending all experiments on a large ſcale, which require repetitions to bring to perfection; or from partial intereſts or prejudices, being oppoſed with ſucceſs (which not unfrequently happens) to improvements of general advantage. And many are apt to ſuppoſe that particular arts and ſciences are brought to the higheſt degree of perfection they are capable of, though experience every day convinces us to the contrary.

The largeſt ſhips might be much more eaſily navigated, if the improvements on capſterns, windlaſſes, blocks, hawſe-holes, &c. were univerſally adopted from the great reduction of the friction.

The following explanation will be eaſily underſtood by thoſe who are acquainted with the conſtruction of a ſhip, See Plate VI.

The courſes and top gallant ſails are to be reefed from the deck, and the top ſails by one man at each lower yard arm.

Deſcription of the improved ſails.

A. The after-part of the ſails.

B. A ſtrong band on the after-part of the ſails, ſewed on at the upper part only, and roped at the lower part.

C. The long clews of the courſe, formed by the bight of the leech rope and rope of the reef band with thimbles, ſeized in above the tack blocks, for laſhing the lower clews to.

D. The tacks and ſheets fitted to the upper clews of the courſes with thimbles above the tack blocks.

E. The buntlines, brought up through the thimbles H, on the foot ropes of the ſails, and bent to the cringles I, on the ropes of the reef bands.

F. A ſmall rope or gaſket, rove, occaſionally as a reef line, through eyelet holes, under the reef bands, and made faſt to the middle ſail, for confining the ſail when reefed, in the wake of the reef bands.

G. Thimbles in the clews and earings.

K. Thimbles on the foot rope with the earings rove through them.

L. The reef tackle pendants, paſſing through thimbles in the clews and leech of the top-ſail, and brought up and bent to the cringles above the upper reef band.

M. A boom tackle or burton hooked to the reef pendants.

N. The crow-foot legs to the top gallant buntline.

N. B. The reef bands are ſewed by the upper part, to the after part of the ſails, to prevent the rope from girting the ſails, when the whole ſail is ſet.

The rope of the reef band of the courſe, is the ſame ſize, as the common foot rope, and the foot rope muſt be in proportion to the rate of the ſhip: for the firſt rates, $3\frac{1}{4}$, or 4 inch; ſecond rates $3\frac{1}{2}$; third rates, 3 inch rope: as the quantity of ſail below the reef band does not require ſo ſtrong a foot rope, as when the whole ſail depended on it.

The rope of the reef bands of the top ſails, ſhould be ſmaller than the leech ropes, as the foot of the ſail will be conſiderably ſtrengthened, when reefed.

Theſe ſails are not ſo heavy as the common ones; a 74 gun ſhip's courſe is reduced in weight about 200lbs. as the points, bands, and eyelet holes of the old reefs are not required, nor any additional geer.

Men of war will find one reef at the foot of the top ſails, very uſeful in chace in ſqually weather, or when obliged to haul ſuddenly on a wind, &c.

Merchant ſhips will only require two reefs in the top ſails, as the ſquareſt part of the ſail is taken off, by reefing at the foot inſtead of the head, but more reefs may be added if neceſſary.

Inſtructions for reefing and ſetting the ſails.

When the courſes are to be reefed, caſt off the lower clews, from the thimbles in the upper clews, haul up the ſlack ſail by the buntlines. and haul tort the reef line, one part at a time, from the middle of the ſail, towards the clews, and make it faſt round the upper clews, ſo as to confine the lower clews.

To ſet the ſail, reeve a few turns of the laſhing for the clews, and haul them down, overhauling the reef line, and buntlines.

To reef the top ſails, ſend a man up to each lower yard arm, ſettle the hallyards, and haul the ſail down by the reef tackles, and paſs the turns of the earings, through the thimbles in the earing cringles, and on the foot of the rope, and make them faſt. Hoiſt the ſail tort up, haul through the ſlack of the buntlines, and haul tort the reef lines on each ſide towards the clews, and make faſt,

The top gallant ſails are reefed from the deck, by the clew lines, and a ſingle buntline with a crow-foot.

The buntlines and reef line will confine the ſlack ſail, when reefed, cloſe up the wake of the reef bands; and the buntlines will only require to be kept hand tort, as is uſual, to prevent them from chafing the ſail.

The ſlack ſail of the roof of the top ſail, will be kept extended tort acroſs the foot, by the reef pendants paſſing through cringles in the leech.

The ends of the clewlines may likewiſe paſs through cringles, in the leech of the top gallant ſails if neceſſary.

The reef lines, if neceſſary, may be in ſeparate pieces, made faſt in the middle and quarters of the ſail.

XI.

Experiments on condenſed Gaſes. By T. NORTHMORE.

To Mr. NICHOLSON,

SIR,

Experiments on condenſed gaſes.

I NOW take the liberty of preſenting you with a continuation of my experiments upon the condenſation of the gaſes, but firſt beg leave to make one obſervation, viz. that the quantity of gas ſaid to be injected in each experiment, cannot (particularly in the preceding article) always be depended upon; for its tendency to eſcape is ſo conſtant and powerful, as frequently to elude every effort of mine to prevent it, and if it can find no other exit, it will ſometimes eſcape by the ſide of the piſton of the forcing pump. In the preceding experiments I have endeavoured as much as poſſible to obviate this evil, but not always with the ſucceſs that I could wiſh.

Nitrogen condenſed upon lime, produced nitrate.

Repeating the eighth experiment mentioned in my former letter, (ſee Vol. XII. p. 372-3) viz. the condenſation of nitrogen upon lime*, in order to diſcover the cauſe of the loſs of colour in the nitrogen, I perceived that this aroſe from its fixation, and a nitrate of lime was the reſult. This experiment, on account of the elaſticity of nitrogen previous to its change of habitude, requires ſome caution; for one of my beſt receivers, three-eighths of an inch thick, was ſhivered in pieces with a violent exploſion, after I had ſet it aſide to ſee the effect of time upon the compreſſed gas.

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* Your marginal note ſays erroneouſly *lime-water*.

Nitrogen and gaſeous oxide of carbon condenſed, gave nitrous acid, &c.

Experiment 9. Upwards of a pint of nitrogen was condenſed, and upon this I pumped one pint of gaſeous oxide of carbon. The colour of the nitrogen was deſtroyed; nitrous acid was formed; and upon collecting the liberated gaſeous oxide, it burnt not unlike alcohol. The two gaſes together were at firſt highly elaſtic.

Exploſions attributed to nitrogen.

From the facility with which nitrogen becomes united and fixed in various bodies, and from its expanſive force when liberated from that ſtate, I know not whether I am ſufficiently warranted in ſuggeſting an opinion, that the exploſive force of various compounds may in a great meaſure be attributed to the ſudden liberation of this fixed gas. To this cauſe I partly attribute the fulminating ſilver of Berthollet; the fulminating gold, and various nitrates; and the detonation which accompanies the decompoſition of ammoniac by oxigenated muriatic acid gas.

Attempt to fire phoſphorus by condenſed air.

Exp. 10. Having been unſucceſsful in my endeavours to inflame phoſphorus by the compreſſion of atmoſpheric air, (ſee *Exp.* 4.) I now tried oxigen, but with little better effect. The phoſphorus appeared to be ſomewhat diſcoloured, and I thought had a tendency to liquify, as it does when put upon a heated plate of iron. Indeed I have no doubt that ſome heat is generated by the condenſation of air, ſince the thermometer riſes upon external application to the receiver.

Oxigenated muriatic acid gas gave a yellow and highly volatile fluid by condenſation.

Exp. 11. Upon the compreſſion of nearly two pints of oxigenated muriatic acid gas in a receiver two and a quarter cubic inches capacity, it ſpeedily became converted into a yellow *fluid*, of ſuch extreme volatility under the common preſſure of the atmoſphere, that it inſtantly evaporates upon opening the ſcrew of the receiver. I need not add, that this fluid, ſo highly concentrated, is of a moſt inſupportable pungency. When atmoſpheric air was pumped into the empty receiver, it was ſpeedily filled with denſe white fumes. There was a trifling reſidue of a yellowiſh ſubſtance left after the evaporation, which probably aroſe from a ſmall portion of the oil and greaſe uſed in the machine, mixed with ſome of the concentrated gas; it yielded to ſulphuric ether, and deſtroyed vegetable colours.

This gas is very injurious to the machine, and on that account difficult to work.

Oxigenated muriatic acid and oxigen afforded a thicker fluid.

Exp. 12. Upon half a pint of oxigen was injected one pint of oxigenated muriatic acid gas. The reſult was a thicker ſubſtance which did not ſo ſoon evaporate, and a yellowiſh maſs was left behind.

Oxigenated muriatic gas and nitrogen.

Exp. 13. Upon half a pint of nitrogen was injected one pint of oxy-muriatic gas. The reſult was a ſtill thicker ſubſtance, and the yellow colour deeper, nor did it appear to act ſo powerfully upon vegetable colours. Much of the greaſe of the machine was carried down in both theſe laſt experiments, which formed part of the yellow reſidue, and yielded only to ether.

Receiver burſt, caution.

Exp. 14. Having condenſed about a pint of carbonic acid, the receiver very unexpectedly burſt with violence. This circumſtance I attribute to the vicinity of the furnace, and I mention it to guard others againſt ſtanding too near a fire in theſe experiments; nor perhaps may it be uſeleſs to add another precaution, that of uſing goggles, or at leaſt a thick plate of glaſs when examining the reſults.

Carbonic acid, and oxigenated muriatic acid.

I now took a new receiver of three cubic inches of capacity, and pumped in one pint of carbonic acid, and upon this rather more than a pint of oxigenated muriatic acid gas.

The union produced a light ſap-green colour, but no fluid, though as uſual the oil of the machine had retained enough efficacy to deſtroy vegetable colours.

Oxigenated muriatic acid gas on hidrogen.

Exp. 15. Upon rather more than a pint of hidrogen, which was highly elaſtic, were compreſſed two pints of the oxigenated muriatic gas. The reſult was a light yellow-green colour, and no fluid. Some ſmoke or vapour ſeemed to iſſue out of the receiver upon turning the ſcrew, and the gas was highly deſtructive of colouring matter.

Muriatic acid gas eaſily made fluid by condenſation.

Exp. 16. I now proceeded to the muriatic acid gas, and upon the condenſation of a ſmall quantity of it, a beautiful green coloured ſubſtance adhered to the ſide of the receiver, which had all the qualities of muriatic acid; but upon a large quantity, four pints, being condenſed, the reſult was a yellowiſh-green glutinous ſubſtance, which does not evaporate, but is inſtantly abſorbed by a few drops of water; it is of a highly pungent quality, being the eſſence of muriatic acid. As this gas eaſily becomes fluid, there is little or no elaſticity, ſo that any quantity may be condenſed without danger. My method of collecting this, and other gaſes which are abſorbable by water, is by means of an exhauſted florence flaſk (and in ſome caſes an empty bladder) connected by a ſtop-cock with the extremity of the retort.

An idea here occurs to me, that the facility of fixation which is the property of the compreſſed muriatic, oxy-muriatic, and ſome other gaſes, may be made of ſome utility to the arts, ſince by previouſly pouring in a little water, or other fluid into the receiver, an acid may be obtained of almoſt any degree of concentration.

Sulphureous acid gas condenſed by preſſure.

Exp. 17. Having collected about a pint and a half of ſulphureous acid gas, I proceeded to condenſe it in the three cubic inch receiver, but after a very few pumps the forcing piſton became immoveable, being completely choked by the operation of the gas. A ſufficient quantity however had been compreſſed to form vapour, and a thick ſlimy fluid of a dark yellow colour began to trickle down the ſides of the receiver, which immediately evaporated with the moſt ſuffocating odour upon the removal of the preſſure. This experiment corroborates the affirmation of Monge and Clouet, mentioned in Accum's chemiſtry, vol. I. p. 319. viz, that " by extreme artificial cold, and a ſtrong preſſure exerted at the ſame time, they rendered ſulphureous acid gas fluid. From the injury which this gas does to the machine, it will be very difficult to perform any experiments upon its elective attractions with the other gaſes.

I remain, Sir,

Your obedient humble Servant,

T. NORTHMORE.

Devonſhire Street, Portland Place,
Feb. 15, 1806.

On

XII.

On the Probability that Muriatic Acid is compoſed of Oxigen and Hidrogen. In a Letter from Mr. J. Martin.

To Mr. NICHOLSON,

SIR,

Facts induced in favour of the poſition, that muriatic acid may be an oxide of hidrogen.

LATE experiments in galvaniſm have furniſhed ſufficient grounds to ſuſpect, that the muriatic acid is an oxide of hidrogen, and I have been ſomewhat ſtrengthened in this ſuppoſition by the well known fact, that hidrogen gas is always liberated upon effecting a ſolution of tin in muriatic acid: this phenomenon has been accounted for, by ſuppoſing the water which held the muriatic acid in ſolution to be decompoſed; its oxigen ſeizing the metal which thereby became diſpoſed to be taken up by the acid and the hidrogen, the other conſtituent part of the water being liberated under the form of gas: however plauſible this hypotheſis might ſeem, I did not think it perfectly ſatisfactory, for if the acid conſiſted of oxigen and hidrogen, part of the oxigen might unite to the metal to render it fit to be diſſolved by the remaining acid, and its hidrogen of courſe given out under the gaſeous form, in this caſe no decompoſition of the water would take place, or at leaſt theſe phenomena might happen without that decompoſition. To clear up theſe doubts I procured an earthern tube into which was introduced ſome iron wire; the tube was made to traverſe a furnace; to the one end was luted a bent tube, brought under the ſhelf of a pneumatic trough, and to the other was adapted a tubulated retort, containing ſome muriate of ſoda carefully freed from its water of cryſtallization When I ſuppoſed the iron wire was ſufficiently ignited, I affuſed ſome denſe ſulphuric acid over the muriate of ſoda; as ſoon as the atmoſpheric air which the veſſel contained was nearly expelled, hidrogen gas was liberated from the other extremity of the tube in conſiderable quantities, mixed however with a ſmall portion of muriatic acid gas; after the operation had been ſuffered to go on ſome time, the apparatus was taken to pieces, and cryſtals of muriate of iron were found in the tube. May

Experiment. Muriatic acid gas diſengaged from decripetated ſea ſalt by ſulphuric acid, was paſſed over ignited iron. Hidrogen was liberated.

It is inferred that this came from the acid.

we not from this experiment be ſufficiently authorized to conclude, that muriatic acid is compoſed of oxigen and hidrogen, and that hidrogen gas is liberated in conſequence of part of the oxigen of the acid uniting to the metal to prediſpoſe it to unite to the remaining acid?

It is to be remarked, that the hidrogen gas was liberated in ſuch abundance as to do away every idea, that it might proceed from any water which the gas accidently held in ſolution.

Should you deem theſe obſervations of ſufficient value, an inſertion of them in your valuable journal will greatly oblige,

Sir,
Your moſt obedient,
and moſt humble Servant,
J. MARTIN.

Crown-Court, Old Bread Street,
February 20, 1806.

XIII.

Subſtance of a Memoir read before the Society of Emulation, at Amiens, by Meſſrs. Reynard *and* Facquer, *on the foul Air of Oil Ciſterns*[*].

Fatal effects of the foul air of an oil ciſtern.

ACHILLE POULAIN, ſoap-maker at Amiens, and one of his workmen having been killed by the foul air of an oil ciſtern, into which the latter had fallen in an attempt to cleanſe it, and the former in endeavouring to ſave the man's life, Meſſrs. Reynard and Facquer were induced to make an analyſis of the deleterious vapour which had cauſed this melancholy accident.

Dimenſions of the ciſtern.

The ciſtern meaſured about twelve feet in every direction. Its mouth is ſecured with a ſmall cover which completely excludes the external air.

Appearance of the oil.

The vegetable oil, of which only a ſmall quantity at a time had been depoſited in this ciſtern, was thick, viſcid, and even in ſome places gelatinous, yielding a ſtrong rancid effluvium.

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[*] Annales de Chimie, Vol. LVI.

A lighted candle on being let down into the ciſtern, was inſtantly extinguiſhed.

The ſurface of lime-water, when included for a few minutes in a broad veſſel, was ſlightly tinged with priſmatic colours.

To obtain the gas for experiments, bottles filled with water were lowered into the ciſterns, and emptied at various depths.

Obſervations on the gas. On the gas obtained from about two feet below the mouth of the ciſtern, the following experiments were tried:

1. A cylindrical veſſel being filled with the gas, kept in contact with lime-water, during fifteen days, with frequent ſhaking, cauſed a ſmall diminution in the bulk of the gas.

2. The ſame experiment repeated with ammoniac offered a ſimilar reſult.

Theſe two experiments denote the preſence of carbonic acid gas.

3. The gas remaining from the two former experiments, when put in contact with liquid hydrogenated ſulphuret of potaſh, underwent an abſorption of eight centimes; which muſt have been oxigen.

It contained carbonic acid. The gas taken within a foot of the bottom of the ciſtern afforded ſimilar reſults, only the proportion of carbonic acid gas was greater. That which remained after the effect of reagents was azote, as the following phenomena prove.

1. A lighted candle was extinguiſhed by immerſion in the gas at the upper part of the cylindric veſſel; but it remained burning if the veſſel was previouſly opened for a few ſeconds.

2. The veſſel when reverſed loſt none of the gas contained in it; and the light was extinguiſhed when introduced.

3. The luminous combuſtion of phoſphorus in oxigen gas (the formation of nitric acid with this gas and oxigen gas not having been tried) was conſidered a poſitive proof of its nature.

This noxious gas was found to contain,

Analyſis.

Upper Part,		Lower Part,	
Azotic gas - -	86	Azotic gas - -	80
Oxigenated gas - -	8	Oxigenated gas - -	6
Carbonic acid gas -	6	Carbonic acid gas -	14
	100		100

Chemical agency inſufficient to deſtroy the foul air.

The nature of this gas does not admit of purification by lime or ammonia. Theſe indeed deſtroy the carbonic acid, but have no influence on the azote.

Mechanical means more effectual.

Mechanical means are the only methods by which any conſiderable quantity of this air can be ſpeedily removed; ſuch as the firing of gun-powder, the uſe of ventilators, &c.

Deſtructive effects of confined air cauſed by the preſence of azotic gas.

The reſult of this analyſis is rather ſurpriſing, as, inſtead of a ſuperabundance of carbonic acid gas, which was ſuppoſed to be the cauſe of the deſtructive effects of this confined air, azotic gas has been found—a gas lighter than atmoſpheric air.

Theory.

The theory of this reſult ſeems to be, that the oil having deprived the encloſed air of its oxigen, leaves only the azotic gas at liberty.

XIV.

Extract from a Memoir, by Meſſrs. FOURCROY *and* VAUQUELIN, *on the Phenomena obſerved in, and the Reſults obtained from Animal Matter, when acted upon by Nitric Acid. Read at the National Inſtitute, by* A. LAUGIER.*

Berthollet's experiments on azote,

THE exiſtence of azote in animal ſubſtances has been determined by the experiments of M. Berthollet, and the diſengagement of this principle, when treated with nitric acid, is among the most uſeful of modern diſcoveries in chemiſtry.

—repeated.

Meſſrs. Fourcroy and Vauquelin, on repeating theſe experiments on muſcular fibre, have added ſome intereſting reſults to this valuable fact.

The following is a ſummary of their experiments, and of the reſults which they obtained.

Nitrous acid with muſcular fleſh gave azote and ſome carbonic acid.

SECT. 1. A mixture of 150 grammes of muſcular fleſh, with an equal quantity of nitric acid, at 32 degrees, and water, put into a mattraſs, and heated till it boiled gently, gave 96 cubic inches of gas, containing nine-tenths of azote, and one-tenth of carbonic acid.

The reſiduum contained fibrous matter, yellow liquor, and a greaſy ſubſtance.

The reſiduum conſiſted of, 1, Matter which had not loſt its original fibrous formation; 2, a yellowiſh liquor; 3, a greaſy ſubſtance, of a yellow colour, which floated on the ſurface of the liquor.

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* *Annales de Chimie*, Vol. LVI. p. 37.

After ſeparating the greaſe, and filtring the liquor, the reſidue was ſubmitted to the following experiments.

To boiling water it gave a yellow colour, and the property of reddening vegetable blues: After waſhing in ſeveral waters, it continued to turn the colour, though it ceaſed to give acidity. Waſhing rendered its colour deeper than at firſt; and when diffuſed in a little water, it ſtill reddened paper of turnſol.

Its ſolution in alcalis was of a deep blood colour. It was precipitated by acids in yellow flakes.

This matter feels fat and pitchy; has a rancid ſmell, and very bitter taſte.

The fibrous matter reſembles fat.

The fuſion and ſwelling which it undergoes when placed on hot coals, the greaſy vapour, and fetid colour, produced by this operation; the ſmall quantity of coal which it leaves, ſhews its reſemblance to fat ſubſtances, notwithſtanding its acidity.

SECT. 2. On a cloſer inveſtigation of the yellow matter, the following characteriſtics and properties were obſerved:

It ſo ſaturated alcalis as nearly to maſk their properties.— Its combinations with potaſh and ammonia lathered like ſoap and water, and are not decompoſed by carbonic acid, but precipitated the ſolutions of mercury and lead in yellowiſh white flakes.

It ſaturates alkalis.

The yellow matter decompoſed alcaline carbonates, in the cold, with efferveſcence, and likewiſe the acetate of potaſh, with the aſſiſtance of water, and a gentle heat.

Decompoſes carbonate.

The authors of the memoir next made uſe of alcohol, and found that the yellow matter was compoſed of a ſmall quantity of fat, which was taken up by the alcohol; and of an acid, which, on account of its colour, they denominated "*yellow acid.*" This acid, when deprived of its fat, which occaſions an alteration in its properties, was of a deeper colour, more readily reddened the paper of turnſol, did not melt in the ſame manner as before, nor exhale the ſame rancid ſmell, but fetid and ammoniacal vapours.

It is a yellow acid and fatty matter.

The yellow acid is diſſolved in the fat, to which it communicated acidity and rancidneſs. It combined with ammonia, and deprived it of its ſmell; and by diſtillation it yielded all the products of animal ſubſtances. Its conſtituent principles, therefore, are azote, hydrogen, carbon, and oxigen; and it muſt be placed among animal acids.

The yellow liquor is an animal acid, conſiſting of azote, hydrogen, carbon and oxygen.

Sect. 3. The combination of yellow acid and fat, on being again ſubmitted to the action of nitric acid, at a temperature of about 50 degrees, underwent no remarkable alteration.—Its colour changed from yellow to white; its ſpecific gravity was diminiſhed, as was likewiſe its bulk; but without any motion or effervеſcence in the acid. Blue colours were deeply reddened by it; it diſſolved, as before, in the ley of potaſh, to which it communicated an orange-red colour, and had an extremely acrid taſte. The action of nitric acid upon this yellow matter ſeems confined to giving it properties which make it approximate to an oily ſtate, without deſtroying its original acid character.

Experiments on the nitric acid wherein the muſcular fleſh had been decompoſed.

Sect. 4. It was of importance that the nitric acid with which the muſcular fleſh had been decompoſed, ſhould be examined. Its yellow colour reſembled that of the ſolution of chromate of potaſh. When ſaturated with carbonate of potaſh, the liquor at firſt acquired an orange colour, afterwards it became turbid, and depoſited a ſmall quantity of orange-red powder. On diſtillation, this mixture afforded a clear liquid, void of colour, of a rancid ſmell, containing a little ammonia, probably formed by the nitric acid. What remained in the retort, was of a blackiſh brown colour, but it was not farther examined.

A colourleſs liquor, having the ſame taſte and ſmell, was afterwards obtained by diſtillation of another portion of the nitric acid uſed in the decompoſition of the muſcular fleſh. The liquor remaining in the retort became yellow by concentration, and its re-action upon nitric acid was quickly perceived in a copious emiſſion of red vapours. When reduced to 40 grammes, flattiſh cryſtals were formed in a thick mother-water, whoſe tenacity was ſimilar to that of the ſolution of gum.

This mother-water poſſeſſed an acid bitter taſte, and on the addition of a little cauſtic potaſh, became of a blood-red colour: mixed with alcohol, it depoſited a white flaky ſediment, which afterwards formed itſelf into fine ſemi-tranſparent grains, of a pleaſant acid flavour.

Five decigrammes of this ſalt, on being calcined, left 21 centigrammes of yellowiſh very light reſiduum, which effervеſced and were diſſolved in nitric acid, and on being evaporated produced cryſtals of ſulphate of lime and nitrate of potaſh.

This ſaline precipitate, obtained by means of alcohol, was aſcertained to be a mixture of ſulphate of lime and acidulous oxalate of potaſh.

The mother-water, after precipitation with alcohol, gave a ſecond precipitate with lime-water, conſiſting of oxalate of lime. After this double operation with alcohol and lime-water, the mother-water, on being gradually evaporated, became converted into the brown viſcid ſyrup, of a bitter taſte, like that of walnut ſhells. This being mixed with a good quantity of alcohol, coagulated, and threw down a plentiful precipitate of white matter. This matter was very pure malate of lime, the alcohol having retained the yellow acrid ſubſtance.

Concluſions.

The learned authors of the memoir, of which we have given this detailed extract, conclude from the facts above ſtated,

1. That the muſcles contain potaſh, lime, and ſulphuric acid, or perhaps ſulphur burned by nitric acid.

2. That a portion of the muſcular fibre, or rather the cellular membrane with which it is enveloped, was converted by the action of the nitric acid into oxalic acid and malic acid.

The alcohol employed in the ſeparation of the malate of lime, held in ſolution, 1, A ſmall portion of nitrate of lime; 2, A very bitter red-brown matter, poſſeſſing the flavour of walnut rhinds, of which more will be ſaid hereafter; 3. A ſmall quantity of that detonating matter already found in indigo: it was in this caſe obtained by concentrating the alcoholic ſolution, and ſeparating it by the addition of carbonate of potaſh, in the form of granulated cryſtals, very inflammable, and very detonating.

Importance of the foregoing analyſis.

SECT. 5. The importance of the reſults obtained from the foregoing analyſis will be readily underſtood; particularly if a compariſon be made of the knowledge hitherto poſſeſſed, with the extenſive notions here opened to the view, of an object ſo intereſting in the conſequences which may be drawn from it, in the applications which may be made to the animal economy, and which, as will be ſhewn, leaves ſcarcely any thing more to be deſired.

Diſcoveries added to what was formerly known on this ſubject.

The diſengagement of azotic gas, the formation of carbonic acid, of fat, of oxalic acid, and of a bitter ſubſtance, conſtitute the whole that was known reſpecting the treatment of animal ſubſtances by nitric acid; to this is now added the diſcovery, 1, Of a yellow inſipid matter, of little ſolubility, though acid, and which immediately ſucceeds the fleſhy fibre; 2, Of another yellow matter, bitter, more ſoluble, and equally acid, which remains diſſolved in the nitric liquor; 3, Of an inflammable, detonating ſubſtance, which is alſo retained in ſolution; 4, and laſtly, of the formation of malic acid.

It appears, and is the opinion of Meſſrs. Fourcroy and Vauquelin, that the yellow and nearly inſoluble matter is the firſt degree of change produced upon the muſcular fibre; it paſſes quickly to the ſecond degree of alteration and of acidity, whoſe product is the more ſoluble yellow matter: this, by a third degree of alteration is ſucceeded by the inflammable detonating ſubſtance, being the third and laſt term of the decompoſing action of nitric acid. The authors of this memoir attribute the ſucceſſive formation of theſe three compounds to the ſubtraction of part of the azote, and of a more conſiderable portion of the hydrogen: by this means the proportions of their elements are changed, and there remains an exceſs of carbon and of oxigen, which produces the ſtate of fat and acidity already noticed. As to the proportion of the conſtituent principles of theſe three compounds, it is a problem of too remote a nature for its ſolution to be readily diſcovered.

Acidity of the yellow ſubſtance not cauſed by nitric acid.

Meſſrs. F. and V. examined if the acidity of the yellow ſubſtances might in any meaſure ariſe from nitric acid; but, after a careful inveſtigation, they were ſatisfied that it was in no degree preſent,

Formation of oxalic and malic acids.

The formation of oxalic and malic acids belongs to the white mucous ſcales of the cellular membrane. Comparative experiments of the effects of nitric acid on the white membranaceous organs, which furniſhed plenty of theſe acids, and very little of the fat yellow matter, led the authors to this concluſion.

Sect. 6. A few inſulated facts, which hitherto have ſcarcely appeared to be ſuſceptible of any uſeful application, ſeem to unite with thoſe preſented by this analyſis; and the learned chemiſts, to whom we are indebted for it, have not omitted to connect them with the other facts. Such are thoſe which are obtained by examining the bilious concretions in certain animals; thoſe in the gall-bladder of the ox and elephant; and the analogy which appears to exiſt between bile, the colour of the ſkin in perſons afflicted with the jaundice, and alſo their urine, and the yellow ſubſtance treated of in this memoir.

Analogy of the yellow matter to bile, jaundice, &c.

New experiments made with a view to confirm theſe ſuſpicions obtained the moſt happy reſults. The red matter of bilious concretions, when ſeparated from the bitter green matter with which it is combined, diſplayed ſimilar properties with the firſt yellow matter obtained from muſcles acted upon by nitric acid.

Bilious concretions.

From the urine of a young man troubled with a ſlight jaundice, they obtained a red ſubſtance, whoſe identity with the matter formed by muſcles and nitric acid was remarkable. To obtain this, they evaporated the urine to the conſiſtency of honey, and treated the reſiduum with alcohol: this contained, beſides much of uree, ſal-ammoniac, and acetate of ſoda, of which the patient made uſe, the red ſubſtance they ſought for.

It was found in the urine of an icteric ſubject.

From theſe experiments, made with ſkill and ability, may we not conclude with the authors, that the jaundice is occaſioned by a ſuperabundance of this matter introduced to the cutaneous abſorbent ſyſtem; that this is what gives a yellow colour to bile and bilious calculi, which diſplay, on analyſis, the ſame properties; and that the yellow acid is diſperſed throughout the animal economy, either by the oxigenation of the muſcular fibre, or of the ſanguineous fibrine, from which it is formed?

Jaundice occaſioned by a ſuperabundance of the yellow acid; which alſo cauſes the yellow colour of bile, &c.

Neither can we avoid admitting a ſtriking analogy between this yellow acid matter, and the acid found in fat after long expoſure to the air, or that has contracted a yellow hue through diſeaſe, and fat treated with nitric acid to form oxigenated pomatum.

Reſemblance of the yellow acid and rancid matter of fat.

It muſt be confeſſed that theſe conjectures aſſume much probability, when we conſider that the acetate of ſoda, alcaline carbonates, and yolks of eggs, are the remedies beſt adapted for the cure of the jaundice, and form alſo the beſt chemical ſolvents of the yellow acid, or of the acid and fat matter, which ſo evidently characteriſe the jaundice.

Other facts.

After what has been ſaid, it muſt no longer be imagined that the hope of tracing the cauſe of morbific affections, is altogether chimerical: nor that diſcoveries in chemiſtry, and attentive reſearches

Chemical reſearches not to be neglected by phyſicians.

reſearches reſpecting animal matter, will not enlighten the phyſician on the nature of diſeaſes, and the means of curing them.

XV.

Remarks relative to Dr. HERSCHEL's *Figure of Saturn.*

By AN OBSERVER.

To Mr. NICHOLSON.

SIR,

Singular circumſtance that Dr. Herſchel's figure of Saturn had not been before obſerved.

ON reading in your Journal, Obſervations on the ſingular Figure of the Planet Saturn, by Dr. Herſchel, from the Philoſophical Tranſactions; when I ſaw the engraving of the figure, as deſcribed by the Doctor, reſembling a parallelogram, one ſide whereof is the equatorial and the other the polar diameter, with the four corners rounded off, ſo as to leave both the equatorial and polar regions flatter than they would be in a regular ſpheroidical figure; I was ſurpriſed to find, on enquiry, that ſo remarkable a figure had not been noticed before by other aſtronomers, whoſe teleſcopes were ſuppoſed to define objects very correctly, with powers conſiderably exceeding 160 times, by which power the Doctor could diſtinguiſh Saturn from the ſpheroidical figure of Jupiter.

Former obſ. of the Doctor did not ſhew it.

In the year 1776, the Doctor relates he perceived the body of Saturn was not exactly round, and in 1781, that it was flattened at the poles, at leaſt as much as Jupiter. In 1789, the Doctor being then prepoſſeſſed with its being ſpheroidical, he meaſured the equatorial and polar diameters, and ſuppoſed there could be no other particularity to remark in the figure of the planet.

It is evident, from the Doctor's former obſervations of Saturn and Jupiter, that the viſible difference in their figures was not, before laſt year, obſerved ſo diſtinctly, owing to the ſuperior excellence of his 10-feet teleſcope of two feet aperture, but that, when obſerved, he afterwards found the other teleſcopes gave a ſimilar diſparity.

Q. whether there was no deception in the teleſcopes.

As the figures given by former aſtronomers, and even by the Doctor himſelf, of both Jupiter and Saturn, were ſpheroidal,

dal, it may be requiſite, before any intricate reſearches are attempted (as mentioned by the Doctor at the end of the communication), to be well aſſured that his teleſcopes have defined the figures of the planets accurately, which at preſent admits of a doubt, and which may be cleared up about the time of the next oppoſition of the Sun and Saturn, in April next.

The following may prove the neceſſity of ſuch an enquiry:

Experiment. An object ſeen by oblique reflection from a ſpherical mirror, is rendered oblong.

Place a circular or ſpherical figure before a concave mirror, which mirror muſt be ſo inclined, that when the object is above the head of the obſerver, it may be ſeen, by reflection, in the center of the mirror[*]: If ſeen within the focus, the object will be repreſented oval in a vertical direction, and when beyond the focus, in a horizontal; which figure will be more and more oval as the angle is enlarged.

Your's,

AN OBSERVER.

XVI.

Experiments on a Mineral Subſtance formerly ſuppoſed to be Zeolite; with ſome Remarks on two Species of Uran-glimmer.

By the Rev. WILLIAM GREGOR.[†]

Deſcription and analyſis of a mineral from Cornwall.

THIS mineral is raiſed in a mine called Stenna Gwyn, in the pariſh of St. Stephen's, in Branwell, in the county of Cornwall; the principal production of which is the compound ſulphuret of tin, copper, and iron.

Deſcription.

Two ſpecies of this mineral are found, aſſuming a marked difference in external character.

The firſt and moſt common one conſiſts of an aſſemblage of minute cryſtals, which are attached to quartz cryſtals, in tufts, which diverge from the point of adherence, as from a centre. Theſe tufts vary, as to the number of cryſtals, of which they are

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[*] If the object is ſmall, it may be enlarged by a concave eye-glaſs.

[†] Phil. Tranſ. 1805.

Deſcription and analyſis of a mineral from Cornwall.

are compoſed, and are light and delicate in the forms which they aſſume, or they are grouped together according to a variety of degrees of proximity and compactneſs. Sometimes they fill the whole cavity of a ſtone, with little or no interruption; in other ſpecimens they are ſeen partially ſpreading over the ſides and pointed pyramids of quartz cryſtals.

In ſome caſes theſe grouped tufts adhere very pertinaceouſly to the ſtone which bears them; in others, they are eaſily ſeparable, in comparatively large pieces, from the quartz, the impreſſed form of which the pieces thus ſeparated retain. The ſurface of theſe, which was in immediate contact with the quartz, exhibits the ſeveral minute cryſtals of which the maſs conſiſts, matted together in various directions.

Theſe cryſtalline aſſemblages are, in general, white; a nearer inſpection of the individual cryſtals proves that they are tranſparent. Sometimes they are ſtained of a yellowiſh hue by ochry water.

The ſize of theſe cryſtals varies conſiderably in different ſpecimens. Sometimes they aſſume the appearance of a white powder raiſed up in ſmall heaps, upon the ſurface of the ſtone, to which they adhere. In other ſpecimens they reſemble a tender down. And the larger ſort varies, in relative ſize, in the proportion, perhaps, in which a human hair, horſe-hair, and a hog's briſtle, ſeverally differ from each other in magnitude. They ſeldom exceed a quarter of an inch in length. The figure of theſe cryſtals is not eaſily aſcertainable, on account of their minuteneſs. By the help of a very powerful microſcope, they appear to conſiſt of four-ſided priſms; where theſe are broken off, the ſection exhibits a rhomboidal, approaching indeed to an elliptical figure, from the circumſtance of the angles of the priſm being worn away; but that the priſm itſelf is rhomboidal, cannot be inferred from hence, unleſs we could be certified, that the ſection were at right angles with the axis of it.

Imbedded amongſt theſe cryſtals two ſpecies of cryſtalline laminæ are frequently diſcoverable; the one conſiſting of parallelopipedon plates with truncated angles, applied to each other, of a green colour of various tints, from the emerald to the apple-green: the other ſpecies, conſiſting of an aſſemblage of ſquare plates, which vary in thickneſs. The angles of the ſeveral ſquare laminæ, which are applied to each other, are

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not always coincident. They are of a bright wax yellow. The ſides of the largeſt of theſe ſquare laminæ is about a quarter of an inch. This laſt ſpecies is frequently found adhering to the ſides of quartz cryſtals, in the cavities of granite.

The other ſpecies of this mineral conſiſts of an aſſemblage of cryſtals cloſely compacted together in the form of mammillary protuberances, in general, of the ſize of ſmall peas, intimately connected with each other. A ſtratum of theſe about ⅛ of an inch thick, is ſpread upon a layer of quartz, in the cavities or fiſſures of a ſpecies of compact granite. The ſtriæ of which theſe mamillæ conſiſt, diverge from a centre, like zeolite. Some of the individual ſtriæ, in ſome caſes, overtop their fellows, in theſe globular aſſemblages, and evidently aſſume, on their projecting points, a cryſtallized form.

A.

(1.) The detached cryſtals of the former ſpecies are eaſily reduced to powder, of a brilliant whiteneſs. At the temperature 56° of Fahrenheit, its ſpecific gravity was found to be 2,22.

(2.) The hardneſs of the more compact ſpecies is ſufficient to ſcratch calcareous ſpar. At the temperature 55°, its ſpecific gravity was 2,253. It does not imbibe water.

(3.) Some of the cryſtals expoſed, on charcoal, to the flame of the blowpipe ſuddenly and ſtrongly driven upon them, decrepitate: if they are gradually expoſed to the flame they grow opaque, and become more light and tender: but they ſhow no ſigns of fuſion under the ſtrongeſt heat.

(4.) The phoſphate of ſoda and ammonia takes up a piece of this mineral without efferveſcence, but it ſwims about the fuſed globule, unaltered. Borax diſſolves a fragment of a cryſtal, and the globule remains tranſparent.

(5.) Some of this mineral, reduced to a fine powder, was mixed with about half its weight of pounded quartz, and kneaded with water into a ball: but as ſoon as the maſs became dry, all coheſion was deſtroyed, and it fell into powder.

(6.) Sulphuric acid, poured upon ſome of it, cauſed no efferveſcence, nor was there any perceptible vapour extricated.

(7.) Some of the pulverized cryſtals were put into a crucible of platina, and ſulphuric acid was poured upon them. The

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crucible was covered with a piece of glaſs, and placed in warm ſand. On examination of the crucible and its contents, after ſome time, it appeared that the greater part of the mineral had been diſſolved, but the ſurface of the glaſs cover was not in the leaſt affected.

(8.) Some of the cryſtals were introduced into a ſmall glaſs retort, to which a receiver was adapted. The retort was expoſed to the heat of a charcoal fire. A fluid diſtilled over into the receiver, which had a peculiar empyreumatic ſmell. It changed litmus-paper to a faint red. It produced no change in a ſolution of nitrate of ſilver; but it cauſed a white precipitate in a ſolution of nitrate of mercury. I attributed theſe phænomena, at the time, to a ſmall bit of the feather with which I had ſwept the powder into the retort, and which, I thought, had fallen into it. A ſlight whitiſh cruſt was alſo produced in the neck of the retort, but the ſmallneſs of the quantity did not admit of examination.

(9.) Some of this mineral, expoſed to a red heat for about ten minutes, loſt in weight at the rate of $25\frac{5}{8}$ *per cent.* Another portion, expoſed to a ſtronger heat for more than an hour, loſt $30\frac{3}{4}$ *per cent.* This operation was performed in a crucible of platina; the cover of which gave ſome indications as if a ſlight portion of the finer parts had been volatilized.

Some of the compact ſpecies, after expoſure to a red heat for one hour, experienced a diminution in weight of 30 *per cent.*

(10.) The ſulphuric, muriatic, and nitric acids, aided by a long digeſting heat, effect nearly a complete ſolution of this ſubſtance. The quantity of the undiſſolved reſiduum is diminiſhed in proportion to the purity of the mineral employed.

(11.) The nitrate of ſilver, as well as the muriate of barytes, produce no change in the ſolution of this ſubſtance in nitric acid.

(12.) The ſolutions of this ſubſtance in muriatic and nitric acids, cannot be brought to cryſtallize.

B.

(1.) I ſelected ſome of the cryſtals of this ſubſtance, as free as it was poſſible from extraneous matter. 50 grains groſsly pounded were expoſed, in a platina crucible, to a red heat for

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one hour. They weighed, *whilſt ſtill warm*, $35\frac{7}{8}$ grains, which is a loſs of $28\frac{1}{4}$ *per cent.* 25 grains of the ſame parcel, from which I had taken the former, expoſed to a heat of longer continuance and greater intenſity, were diminiſhed in weight, at the rate of $30\frac{1}{4}$ *per cent.*

(2.) The powder ſtill preſerved its pure whiteneſs. It was transferred into a matraſs, and nitric acid poured upon it, which ſoon began to act upon it. The matraſs was placed, for many hours, in a digeſting heat. A ſolution of the whole of the ſubſtance, except a ſmall portion, was effected. I added a few drops of muriatic acid, and continued the digeſtion.

(3.) The acid was now diluted with diſtilled water, and poured off from the reſiduum, which conſiſted partly of a fine ſpongy earth, and partly of fragments of quartz. It was caught on a filter and ſufficiently edulcorated. The laſt portion of edulcorating water dropped through the filter of an opaliſh hue.

The reſiduum, dried and expoſed to a red heat, for ten minutes, = $\frac{3}{16}$ of a grain, $\frac{1}{16}$ of which conſiſted of fragments of quartz, $\frac{1}{32}$ was found to be ſilica, and $\frac{3}{32}$ alumina.

C.

(1.) The clear ſolution and edulcorating water were poured into a large matraſs and boiled, and whilſt boiling, the contents were precipitated, in white flakes, by ammonia

(2.) When the ammonia had ceaſed to produce any further precipitate, the clear fluid was decanted, and aſſayed with carbonate of ammonia. But its tranſparency was not in the leaſt diſturbed.

(3.) This clear fluid, together with the edulcorating water, with which the ſubſided precipitate had been waſhed, was gradually evaporated. When its volume was conſiderably diminiſhed, a ſeparation of a ſpongy earth took place, more copiouſly than I had reaſon to expect, and the quantity of it was ſtill further increaſed by a few drops of ammonia. This earth, thus ſeparated, was ſufficiently edulcorated, and added to the former precipitate.

(4.) The fluid was again evaporated, and at laſt transferred to a crucible of platina, and the ſalt reduced to a dry ſtate: on rediſſolving this ſalt in diſtilled water, a minute portion of

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earthy matter was ſeparated, which, after edulcoration, was added to the reſt. The fluid from which it had been ſeparated, and the edulcorating water, were again evaporated to dryneſs, and the ammoniacal ſalt expelled by heat, in a platina crucible.

(5.) After the crucible had been made red hot, it was examined. I diſcovered on the bottom of it, ſome traces of earthy matter, and ſome ſpots, which had a glaſſy appearance. Water boiled upon it, diſſolved nothing; from which circumſtance, the abſence of both of the fixed alkaline ſalts may be inferred. Neither did nitric acid produce any alteration. A few drops of ſulphuric acid effected a ſolution of the ſubſtance, which adhered to the bottom of the crucible. Ammonia precipitated from it a ſmall quantity of earth, which was transferred to the reſt, and the ſulphate of ammonia and edulcorating water were again evaporated and expelled by heat. A few ſpots of the ſame glazing ſtill appeared. I had obſerved the ſame phenomenon in a former experiment: but in that, as well as in the preſent inſtance, the ſubſtance was in too ſmall a quantity to become the ſubject of experiment.

D.

(1.) Upon the precipitate (C 1), and the earths collected at different times, whilſt they were in a moiſt ſtate, I poured a ſolution of potaſh in alcohol mixed with diſtilled water; in a ſhort time, the greater part of it was diſſolved.

The clear ſolution was decanted, and the undiſſolved ſediment was transferred to a baſon of pure ſilver, and boiled with a ſolution of potaſh.

(2.) When the potaſh ceaſed to act upon it, it was diluted with diſtilled water and decanted from a brown powder, which had ſubſided. This powder edulcorated, dried, and ignited weighed $\frac{7}{16}$ of a grain; $\frac{1}{4}$ of a grain was alumina, $\frac{3}{32}$ ſilica, and $\frac{3}{32}$ oxide of iron.

E.

(1.) The ſolution effected by potaſh was decompoſed and rediſſolved by muriatic acid, and the contents of the ſolution were precipitated by ammonia. The ſubſided precipitate was edulcorated.

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(2.) The fluid and the edulcorating water were evaporated to dryneſs, and rediſſolved in diſtilled water. Here again, to my ſurpriſe, a ſeparation took place of a white earth, more abundant than is uſual in caſes where ammonia is employed as a precipitant.

(3.) This earth and the precipitate were edulcorated with diſtilled water, until it ceaſed to affect a ſolution of nitrite of mercury. Collected, dried, and ignited, for one hour it weighed *whilſt ſtill warm* 32 $\frac{1}{16}$.

F.

(1.) This earth was placed in a crucible of platina, and repeatedly moiſtened with ſulphuric acid, which was abſtracted from it in the ſand bath; diſtilled water affected the ſolution of the whole, except a white powder which weighed, after ignition, 2$\frac{1}{32}$ grains. It was proved to be ſilica.

(2.) This ſolution was now mixed with ſome acetat of potaſh and gradually evaporated; large and regular cryſtals of alum were from time to time formed. A ſmall portion of ſilica which weighed after ignition $\frac{1}{32}$ of grain was depoſited; ſome ſulphate of lime alſo made its appearance, which waſhed with diluted alcohol and dried in a low heat $\frac{7}{16}$ of a grain.

(3.) A portion of the fluid remained which neither the addition of potaſh nor the lapſe of many weeks could induce to cryſtallize. Suſpecting that it might contain glucine, I precipitated the contents by carbonate of ammonia, added to exceſs, and ſhook the mixture repeatedly and ſtrongly. The precipitated earth was collected and the fluid boiled, but it was found to contain nothing but a minute portion of alumina.

(4.) The edulcorated earth was rediſſolved in ſulphuric acid, except $\frac{5}{8}$ of a grain of ignited ſilica.

The ſolution was mixed with a little potaſh, and gradually evaporated. Sulphate of lime was ſeparated at ſeveral times and after long intervals, which ſufficiently waſhed and dried in a low heat = $\frac{9}{32}$. Some ſilica alſo ſeparated, but too minute in quantity to be aſcertained by weight. The remaining fluid at length cryſtallized into regularly formed alum.

(5.) The whole, therefore of the 32 $\frac{1}{16}$ (E 3.) conſiſted of alumina except 2$\frac{7}{8}$ of ſilica, and the lime contained in $\frac{23}{32}$ of ſulphate of lime, which may be eſtimated about $\frac{3}{16}$ of a grain; the alumina, therefore, = 29; the alumina in B. and D. = $\frac{11}{32}$

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the ſilica in B, D, and F, $= 3\frac{1}{16}$; the oxide of iron (D.) $= \frac{3}{32}$, and lime F, $\frac{3}{16}$; the volatile parts of this ſubſtance $= 15\frac{3}{8}$ in the 50 grains employed.

The ſum total of theſe is - - -	$47\frac{1}{16}$
Loſs - - - - - - -	$2\frac{15}{16}$
	50

I have ſubjected theſe cryſtals, as well as the harder ſpecies of this mineral, to analyſis by means of direct ſolution in ſulphuric acid, and have found in each caſe the ſame fixed ingredients, viz. alumina, a ſmall portion of ſilica, and a very minute quantity of lime. Both theſe latter ingredients are, I think, eſſential to the compoſition of this foſſil, as I have always diſcovered them in the pureſt ſpecimens. In this mode of analyſis I experienced the ſame difficulty and tedioufneſs of delay in bringing the laſt portions of the ſolution to cryſtallize into alum. This anomalous circumſtance I have reaſon to attribute to a particular combination, which takes place between the ſulphate of alumina and lime, ſilica, and potaſh. In my examination of the compact ſpecies there was no appearance of the ſulphate of lime until the laſt; and in every experiment, previouſly to the freſh appearance of cryſtals of alum that had been long delayed, ſilica and ſulphate of lime were depoſited.

I forbear entering into any further details concerning my former experiments on this curious foſſil, as I have reaſon to think that it will ſtill require a more particular and minute examination, on account of another ingredient which eluded my notice, and which may poſſibly impart to its peculiar character. The ſcarcity of it has been hitherto a great bar to my experiments; I ſhall record, however, a few facts which I have lately obſerved, in the hope that at a future time I may be able to reſume my examination of it.

I was induced to pay more attention to the volatile ingredients of this ſubſtance *. With this view, I introduced ſome

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* Mr. Humphry Davy, whoſe well known ſkill and ſagacity have probably rendered the reſearches of another perſon ſuperfluous, had, I found, been engaged in the analyſis of a mineral which is thought to be identical with the ſubject of theſe obſervations. He informed me that he had obſerved a peculiar ſmell, and acid properties in the water diſtilled from the ſubſtance which he examined.

of the cryſtals into a ſmall retort, adapted a receiver unto it, and expoſed the retort to a charcoal fire. The neck of the retort was ſoon covered with moiſture, which paſſed into the receiver; and I obſerved a white cruſt gradually forming in the arch and neck of the retort.

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On examination of the fluid in the receiver, it was found to have the ſame empyreumatic ſmell that I had obſerved before. It reſembles very much the ſmell which that fluid is found to have which is diſtilled from the white cruſt that ſurrounds flint as a nucleus.

It changed litmus paper to a faint reddiſh hue. It produced no change on a ſolution of nitrate of ſilver, and ſcarcely a perceptible one, on that of nitrate of mercury.

The cruſt formed in the neck of the retort conſiſted of thin ſcales, which after the veſſel had been dried, were diſpoſed to ſeparate from the glaſs in ſome places, but in others they firmly adhered unto it. They were opaque, like white enamel, and reflected the colours of the rainbow. A portion of this ſubſtance expoſed to the flame of the blow-pipe upon charcoal turned at firſt black, and then melted into a globule, that exhibited ſomewhat of a metallic ſplendor which ſoon grew dull. This ſubſtance is ſoluble in water; on evaporation of it, it aſſumes, at the edges of the fluid, a ſaline appearance, which, as the moiſture evaporates, becomes earthy, opake, and white. Some of the ſolution changed litmus paper to a faint red. Lime and ſtrontian waters produce in it white clouds, which a drop of nitric acid removes. Muriats of lime and barytes produce no change in it. Nitrate and acetate of barytes diſturb its tranſparency, the effect produced by the latter is more evident. Nitrate of ſilver produces no effect, but nitrates of mercury and lead cauſe copious precipitates, which are white and ſoluble in nitric acid. Phoſphate of ammonia and ſoda produced a white precipitate. Oxalate, tartrite, and pruſſiate of potaſh did not affect it, nor did ſulphate of ſoda. Ammonia was dropped into it, but the fluid preſerved its tranſparency. But carbonate of ammonia inſtantly cauſed a white precipitate, which was not rediſſolved by an exceſs of the precipitant; upon ſome of this ſubſided precipitate a concentrated ſolution of potaſh was poured and ſhaken with it, but it was not

ſenſibly

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ſenſibly diminiſhed. But if after edulcoration it be diſſolved in nitric acid, and potaſh be added, no precipitate is produced.

Carbonate of potaſh cauſes a white precipitate when dropped into the aqueous ſolution of the ſcaly ſublimate.

The ſupernatant fluid was poured off and gradually evaporated, but it became repeatedly turbid, nor could I by means either of the filter or alcohol prevent a recurrence of the ſame effect. Nearly the ſame reſult takes place when carbonate of ammonia is uſed as the precipitant.

Some of the white ſcales were moiſtened with ſulphuric acid. No vapour aroſe.

Some of the precipitate obtained by means of carbonate of potaſh from the watery ſolution of this ſubſtance, was, after ſufficient edulcoration, diſſolved in ſulphuric acid; the ſolution, on due evaporation, produced permanent cryſtals, ſome of which reſembled alum, but others ſeemed to differ from it in external character. Ammonia decompoſed the ſolution of them in water, and a few drops of liquid potaſh diſſolved the precipitated earth. The quantity was too ſmall for further experiment.

If diſtilled water be poured into the retort and boiled in it, ſo as to diſſolve what adheres to the neck and cavity of it, a further ſolution is effected, but differing in ſome meaſure from the ſolution of the ſublimate collected from the neck of the veſſel. This latter ſolution is found to contain lead. If nitric or muriatic acid be poured into the retort, ſo as to diſſolve what *ſtill* remains adhering to it, the preſence of lead becomes more evident. Whence does this metal ariſe? I have reaſon to believe that it ariſes from the glaſs retort, which is corroded by the acid of the foſſil extricated by heat. But what acid is it? It does not ſeem to be either the phoſphoric or fluoric acids, the latter of which became the firſt object of my ſuſpicion.

The opinion which Mr. Davy ſuggeſted to me ſeems more probable, that it is of vegetable origin. Oxalic acid, on the authority of Bergman, may be volatilized; yet ſome of its properties are very extraordinary and do not accord with this idea.

I decompoſed the watery ſolution of the ſcales by nitrate of lead, and after a ſufficient edulcoration of the ſubſided precipitate, I dropped upon it ſome ſulphuric acid. No fumes were perceptible. The ſulphate of lead was ſeparated by the filter, and the clear fluid, which paſſed through it, was gradually evaporated; ſmall cryſtallizations were formed, the figure of which I could not aſcertain; ſome of them were expoſed to the flame of the blowpipe in a gold ſpoon; they did not burn to coal, nor give out any empyreumatic ſmell nor fuſe, but they aſſumed an earthy appearance *.

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Uran-glimmer.

I ſhall add a few deſultory remarks upon the yellow and green cryſtals, which frequently accompany the foſſil.

I conſidered them to be the two ſpecies of uran-glimmer which had been examined by the celebrated Klaproth.

The yellow cubic cryſtals are light. Their ſpecific gravity, taken at temperature 45° Fahrenheit, was 2,19.

Expoſed to the flame of the blowpipe on charcoal, they decrepitate violently. A piece of this ſubſtance is taken up by phoſphate of ammonia and ſoda, without efferveſcence, and communicates a light emerald-green colour to the fuſed globule.

By expoſure to a red heat, this ſubſtance loſes nearly a third part of its weight. It then becomes of a braſſy colour.

It is ſoluble in the nitric and muriatic acids; but I could procure no cryſtallized ſalt from the ſolution of either of them.

By evaporation to dryneſs, and rediſſolving the maſs, ſome ſilica is ſeparated.

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(1.) A certain quantity of the yellow cryſtals were diſſolved in nitric acid. Muriatic and ſulphuric acids ſucceſſively dropped into the ſolution produced no ſenſible change. The contents of the ſolution were precipitated by ammonia,

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* I ſubjected ſome of the Barnſtaple mineral, with which Mr. Raſhleigh kindly furniſhed me out of his cabinet, to experiment, with a view of aſcertaining whether it would produce the ſame volatilized ſaline cruſt, as the ſtenna gwyn foſſil, and I found that it did.

in

Deſcription and analyſis of a mineral from Cornwall.

in white clots, mixed with ſome of a yellowiſh hue. Ammonia, added in exceſs, betrayed no ſign of the preſence of copper.

(2.) The ammonia, on evaporation, was found to have held a portion of the mineral in ſolution. A freſh portion of ammonia diſſolved more, but in a leſs quantity, at each ſucceeding affuſion of it.

(3.) The precipitate, which had reſiſted the ammonia, was boiled in a ſilver crucible, with a ſolution of potaſh in alcohol, diluted with diſtilled water, and a conſiderable portion of the ſubſtance was diſſolved by it: the potaſh and the ammonia had diſſolved rather more than half of the fixed ingredients of it.

(4.) The edulcorated reſiduum, which was of a dirty yellow colour, was transferred to a crucible of platina, and moiſtened with ſulphuric acid, which was abſtracted from it, in the ſand-bath. The browniſh-gray maſs was elixated with diſtilled water, which diſſolved nearly the whole of it. The reſiduum conſiſted of a white heavy powder, which, tried in different ways, was found to be *ſulphate of lead.*

(5.) The ſolution effected by ſulphuric acid was greeniſh. On evaporation, a ſalt was produced, of uncommon brilliancy, reſembling ſcales of mica, or ſilver leaf. Theſe diminiſhed in quantity at every freſh ſolution and evaporation, and at laſt they could not be reproduced; but a confuſed cryſtallized maſs remained. How far the platina crucible may have contributed to this phenomenon I cannot aſcertain.

(6.) The ſolution of the ſaline maſs was precipitated by potaſh, of a dark brown colour. The potaſh held nothing in ſolution. I rediſſolved the precipitate in nitric acid, and precipitated the ſolution by ammonia, of a bright yellow colour, peculiar to the *oxide of uranium,* with which it agreed in other properties.

(7.) What was diſſolved by ammonia (2) amounted to nearly $\frac{1}{8}$ part of the fixed ingredients. It was white, inclining to aſh-colour. It tinged phoſphate of ſoda and ammonia of a light green It was ſoluble in ſulphuric acid, except a few gelatinous flakes. The ſolution was greeniſh; gradually evaporated, it ſhot into a number of minute ſtellated cryſtallizations, which were circular, and conſiſted of rays diverging from a centre. They were, in general, colourleſs: a few of

them

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them were tinged of a ſmoke-colour. They ſoon became deliqueſcent. Upon evaporation, the ſame cryſtallizations were produced. After a time, ſome detached, regular, and permanent cryſtals were formed, which were colourleſs. Their figure I could not accurately aſcertain. They were expoſed to a red heat in a platina crucible. No ammoniacal vapour was perceptible. The cryſtals melted into opaque globules: ſome of theſe were transferred to a ſmall glaſs, and diſtilled water was poured upon them. No ſolution took place apparently: on ſhaking the glaſs, the globules fell to pieces into gelatinous flakes, which were white. Some of the ſupernatant fluid was tried with muriate of barytes, which produced a cloud. But neither ammonia nor pruſſiate of potaſh cauſed any change in it. It is ſoluble alſo in nitric acid: the ſolution formed a confuſed cryſtallized maſs, which ſoon became deliqueſcent. Zinc, immerſed in it, cauſed the ſeparation of white gelatinous flakes. Iron cauſed no change. Ammonia and potaſh threw down white precipitates, a portion of which were rediſſolved. The carbonates of ſoda, potaſh, and ammonia produced white precipitates. Pruſſiate of potaſh threw down the contents of the ſolution in diſtinct flakes, of the colour of mahogany; and the ſolution of galls in alcohol cauſed a light yellow powder to ſubſide. It is ſoluble alſo in muriatic acid; the ſolution is a very dilute green. It requires an exceſs of acid to hold the ſubſtance in ſolution: which, after a time, depoſits cryſtalline grains of a yellowiſh colour, which require a large quantity of water to diſſolve them.

Acetic acid does not diſſolve this powder.

(8.) What was diſſolved by potaſh (3.) was of an iſabella colour; it was tried with nitric, muriatic, and ſulphuric acids, neither of which could diſſolve the whole of it. What reſiſted the two former acids was found to be ſilica. That which remained undiſſolved by the latter, was ſilica and ſulphate of lead. Evaporation of the latter ſolution, betrayed alſo the preſence of lime, in the ſtate of ſulphate. The nitric and muriatic ſolutions, on evaporation, depoſited nitrate and muriate of lead; and ſulphuric acid dropped into them produced a ſmall quantity of ſulphate of lime.

The nitrate and muriate of lead were decompoſed by ſulphuric acid, and the lead reduced on charcoal.

Ammonia

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Ammonia precipitated what remained in theſe ſolutions, and rediſſolved a part of the precipitates, which agreed in properties with that ſubſtance before mentioned (2.); the remainder was of a brighter yellow. But I could not bring the ſolution of it in nitric acid to cryſtallize.

B.

(1.) Some of the yellow cryſtals, which had not the ſlighteſt appearance of being contaminated with extraneous matter, were diſſolved in ſulphuric acid. *Silica was ſeparated;* and the preſence of *lime* and *lead* proved by the appearance of their reſpective *ſulphates*.

(2.) If ſulphate of ammonia is dropped into a ſolution of this mineral in nitric or muriatic acids, no change takes place, *immediately*. But on evaporation, a yellowiſh cruſt is depoſited, which is inſoluble in water. A ſolution of carbonate of ſoda in water, boiled on it, becomes yellowiſh-brown, and the greater part of it is diſſolved. The reſiduum, which is white, is reduced on charcoal to a globule of lead. What the carbonate of ſoda had diſſolved was found to be *oxide of uranium*. Sulphuric acid *alone*, does not produce this depoſited cruſt.

(3.) Some perfectly pure cryſtals were diſſolved in muriatic acid. Some ſilica was ſeparated. A few drops of ſulphuric acid were dropped into the ſolution, which produced no immediate change. on evaporation a white powder, ſeparated, which conſiſted in part of ſulphate of lime. The remainder, expoſed to the flame of the blowpipe, was reduced to globules of lead.

The ſolution was decompoſed by ammonia, which rediſſolved a part of the precipitate; and, after edulcoration, the precipitate was diſſolved by nitric acid, and precipitated again by ammonia, which held a leſs quantity in ſolution. The edulcorated precipitate was now boiled with a ſolution of carbonate of ſoda, which diſſolved a large portion of it. The ſolution was yellowiſh-brown, and contained oxide of uranium. What was undiſſolved by the carbonate of ſoda was diſſolved in ſulphuric acid, and ſeemed to be the ſame ſubſtance as that which the ammonia held in ſolution, A. (2.)

The ſcarcity of this beautiful mineral has precluded me from operating on ſuch a ſufficient quantity, as a regular and rigid analyſis required.

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The ſubſtance, which is held in ſolution by ammonia, has ſome peculiar properties that ſeem to diſtinguiſh it from uranium. And if this mineral be the uran-glimmer, I have certainly detected the oxide of lead, lime, and ſilica in it, which have not hitherto been conſidered as ingredients of that foſſil. The green cryſtals differ in no reſpect from the yellow, except in containing a little of the oxide of copper.

XVII.

Examination of different Methods of ſeparating Nickel from Cobalt. By M. C. F. Bucholz.*

THE want of nickel and cobalt in a ſtate of purity induced M. Bucholz, to make experiments himſelf on the means of procuring them, and to repeat thoſe of others.

A. The able chemiſt Hermſtadt propoſed to ſeparate oxide of cobalt and oxide of nickel, by diſſolving the nitrate or ſulphate of cobalt, impregnated with nickel, in ammonia, and expoſing the ſolution to a ſingle evaporation. This M. Bucholz tried in the following manner, for the reverſed purpoſe.

M. Bucholz repeats Hermſtadt's method.

1. An ounce of cobalt ore *(cobalt ſpeiſe)* was diſſolved with heat in four ounces of nitric acid of the ſpecific gravity 1,220, and mixed with an equal quantity of water; which produced a reſidue of three drams of oxide of arſenic, in the form of ſmall cryſtals. When the ſolution mixed with half the quantity of water, coloured of a dull green, had been filtered and diluted with a great quantity of water, it depoſited a little of the oxide of biſmuth. Cauſtic ammonia was then mixed with it to exceſs, until no farther apparent ſolution took place of the precipitate obtained. That which was not diſſolved, of a dull reddiſh white, was a compoſition of arſeniate of cobalt with a little of the oxide of biſmuth, and the oxide of iron.

One oz cobalt ore diſſolved in nitric acid.

Depoſits 3 drams of arſenic.

The ſolution filtered and diluted depoſits a little biſmuth oxide.

Cauſtic ammonia added.

The undiſſolved reſidue is arſeniate of cobalt, with oxides of biſmuth and

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* Bucholz, &c Journal of Chem. III. p. 2.

The

By evaporation oxides of cobalt and nickel are precipitated.

The ſolution being filtered, appeared of a beautiful blue, it was then evaporated at a gentle heat, by which about two drams of a bright green precipitate were obtained; which proved to be oxide of nickel, united to oxide of cobalt. The filtered liquor being then afterwards evaporated at the heat of a ſtove, depoſited ſtill an oxide of the ſame quality.

The ſaline maſs obtained, re-diſſolved, filtered, and boiled with cauſtic potaſh produces pure oxide of nickel.

The ſaline maſs of ammoniacal nitrate of nickel, of a deep green colour, which had been obtained by the evaporation, was re-diſſolved, filtered, and kept in ebullition with an exceſs of cauſtic potaſh, until the evaporation of the ammonia was compleated, by means of which a dram and half of oxide of nickel was ſeparated, which did not appear to contain any more oxide of cobalt.

Gives an odour of oximuriatic acid.

Treated with ammonia depoſit oxide of cobalt.

The ſolution cryſtaliſed.

2. As the ſeparation was not effected very well nor with much facility by the former method, the effect of ſulphuric acid was tried. For this purpoſe, an equal quantity of water was poured on the oxide obtained as before, and ſulphuric acid added till all was diſſolved by the aid of heat. It then evidently gave out an odour ſimilar to that of oximuriatic acid, although there was not any muriatic acid uſed. A like phenomenon, on a ſimilar occaſion, was before obſerved by the author (which is mentioned in the firſt ſection, page 18, of *Deitrage zur erweiterung*, for 1799.) The ſolution was then treated with ammonia as before, until the whole was almoſt diſſolved. The reſidue, which was oxide of cobalt with a little oxide of nickel, had the colour of verdigris. When the ſolution was evaporated at a gradual fire, and ſeparated by filtration from the precipitate, of which the greateſt part was oxide of cobalt, it was ſubmitted to ſpontaneous evaporation: It then cryſtalized without any farther ſeparation, partly into priſmatic cryſtals in groups, and of a green colour, and partly into cruſts united together, and blue at the edges; the eſſay of the oxides procured by potaſh from the ſolution of the cryſtals, as well as from the mother water, ſhewed that they contained cobalt almoſt in equal proportions.

The cryſtals contain cobalt and nickel.

The laſt experiment repeated on a larger ſcale

3. Mr. Bucholz repeated the former experiments on a larger ſcale, in hope to obtain a better cryſtallization, and operated on eight ounces of cobalt ore, from which the firſt cryſtals, of a blueiſh green, obtained by a proceſs ſimilar to that laſt recited, and which weighed about five ounces, were again diſſolved in 32 ounces of boiling water: This ſolution was eva-

porated till a pellicle was formed, and, after being filtered, was left near a ſtove, that it might cool ſlowly and cryſtallize. At the end of 48 hours, the greateſt part of the ſalt was cryſtallized in beautiful tetrahedral rhomboidal pyramids, ſhort, and of a yellow green, of which the lateral faces formed angles of 115 and of 65 degrees, often with one extremity truncated; and always with an angle of 132 degrees towards its terminating face. This reſult proves that this ſalt forms more readily into regular cryſtals by cooling than by ſlow evaporation. All the cryſtals were then collected, waſhed with water, and again diſſolved, and the nickel ſeparated by boiling the ſolution with potaſh till the ammonia was diſengaged.

Produces fine cryſtals in tetrahedral rhomboidal pyramids.

The cryſtals diſſolved and the nickel ſeparated as before.

4. As well to free this oxide from carbonic acid as to judge if it had been purified from cobalt, it was diſſolved in nitric acid and treated with pure ammonia in the ſame manner as has been deſcribed. The liquor of a fine blue colour, (and from which a reſidue of five grains, which ſeemed to be an oxide of cobalt, had been ſeparated by filtration), was evaporated to dryneſs. After another ſolution then made, it depoſited an oxide of a beautiful bright green, which, after being waſhed and dried, weighed half an ounce. The liquor, which paſſed the filter, was analyſed by pure carbonate of potaſh at the heat of boiling water, which then produced 170 grains of oxide of nickel, of a pale green, united to carbonic acid; a little of it was diſſolved in muriatic acid, and ſome of the ſolution ſpread upon paper. On heating it afterwards, the tint became yellow, and inclined but very little to a green. But the oxide of nickel, which ſeparated ſpontaneouſly during the evaporation, was diſſolved in diſengaging much oximuriatic acid; ſpread on paper, it exhibited the colour, when heated, of a ſympathetic ink of cobalt highly ſaturated; from whence it follows that it was more rich in cobalt than that procured from the precipitation.

The oxide obtained diſſolved in nitric acid and treated with ammonia, evaporated and re diſſolved depoſits a green oxide.

The filtered liquor yields by potaſh 170 grs. oxide of nickel.

Which contains leſs cobalt than the ſpontaneous precipitate.

The oxides collected in thoſe two ways, diſſolved in nitric and ſulphuric acids, after becoming grey, (which the author ſuppoſed to be occaſioned by the nickel diſſolving firſt, and at leaſt the greateſt part of the cobalt remaining to the laſt, but which opinion was not confirmed by other experiments made on this ſubject.) Theſe oxides made lightly red in the fire, changed their colour to a dark grey, and then, as well as on

The oxides diſſolve in nitric and ſulphuric acids.

Give out nitrous acid by fire, and by ſulphuric acid.

on the addition of ſulphuric acid, a diſengagement of nitrous acid took place from the reſidue obtained by evaporation, which was alſo cauſed by the addition of an alkaline lixivium : With ammonia the ſame effects were produced which have been before mentioned.

Sulphites and nitrates of ammoniacal nickel always contain cobalt.

The reſults of the foregoing experiments are :—The ſulphates and nitrates of ammoniacal nickel ſeparated from cobalt ore, retain always ſome cobalt in their compoſition, and it is impoſſible from the method of Hermſtadt modified in the preceding manner, to obtain an oxide of nickel without a mixture of cobalt.

The oxide of nickel remaining in the ſalt after evaporation contains very little cobalt.

b. By partially decompoſing the ammoniacal nitrate of cobalt by evaporation, an oxide of nickel is obtained, very rich in cobalt, which contains nitric acid; and the oxide of nickel which remains undecompoſed in this ſalt, retains a very ſmall quantity of cobalt.

Dr. Schnaubert's method of obtaining pure oxide of nickel.

B. Doctor Schnaubert has publiſhed (in Tromſdorf's Journal of Pharmacy, vol. II. p. 66) a method of obtaining the oxide of nickel pure: Which conſiſts in diſſolving the metal of nickel mixed with cobalt, or its oxide ſeparated from other ſubſtances, in nitric acid, in precipitating it by the carbonate of potaſh, and in heating it to a white heat, after waſhing and drying it. In this manner he always procured a yellow oxide, on which he cauſed very ſtrong ſulphuric acid to boil; which gave him a ſolution of oxide of nickel of a graſs green; while the oxide of cobalt appeared in the form of a yellow reſidue. He proves the purity of the ſulphate of nickel prepared in this manner, by the property which ammonia has of precipitating it of a bright green, and when added to exceſs, of re-diſſolving it with a beautiful deep blue colour; but this argument appears inſufficient to thoſe who know that oxide of nickel, although mixed with many hundredth parts of cobalt, does not, however, experience any perceptible change in the colour of its precipitates, nor in its ammoniacal ſolutions. Beſides the omiſſion of indicating the means by which he was convinced that the oxide, which was the reſidue of the ſulphuric acid ſolution, was really an oxide of cobalt, with the vague precept of heating the oxide acquired, without the leaſt direction relative to the degree of the fire, and the uncertainty which he leaves of the degree of ſtrength of the

His teſt of its purity defective.

He has not mentioned his proof that the other oxide obtained was cobalt, —nor the degree of heat to be uſed, —nor the ſtrength of the ſulphuric acid employed.

ſulphuric acid which he uſed, altogether throw doubts on the exactneſs of the proceſs indicated, which the following experiments may elucidate.

Experiment on Dr. Schnaubert's proceſs.

1. A portion of the carbonic oxide of nickel, A 1, was expoſed during an hour in a ſtrong fire to a red heat approaching white heat. The oxide while hot was of a browniſh yellow; after cooling it aſſumed a grey colour inclining to yellow, but not yellow. The oxide obtained by the evaporation A 4, having been treated in the ſame manner was ſtill a little more grey than the preceding. The carbonic oxide of nickel was placed again for half an hour in a white heat; while hot it was yellow inclining to brown, but when cool, it was grey inclining to browniſh yellow.

2. Thirty grains of this oxide made red (hot), were put for ſome hours to digeſt, with ninety grains of pure ſulphuric acid of the ſpecific gravity 1,860. Being then heated, the maſs immediately ſwelled up with an exploſive noiſe, and exhibited a yellow ſubſtance inclining to a green; by means of ebullition with half an ounce of water it was diſſolved, except about a grain of a yellowiſh-grey powder, which proved to be an oxide of nickel mixed with cobalt and a little dirt. Thirty-five grains of oxide of nickel, (obtained by heating briſkly to redneſs 60 grains of ammonical nitrate of nickel prepared by evaporation), afforded the ſame reſult, and the ſame phenomena, on being treated in the ſame manner: The ſame oxide being heated for half an hour to whiteneſs, uſing the bellows at the ſame time, did not afford a yellow maſs, but one of a yellowiſh grey inclining to a green, which had the ſame effect with ſulphuric acid that has been already related.

The experiment repeated with diluted ſulphuric acid.

3. The experiment was again repeated with diluted ſulphuric acid; 160 grains of ammonical oxide of nickel, which had been precipitated from many ſolutions were expoſed for half an hour to the moſt violent white heat, under the operation of the bellows, after which they weighed 75 grains. This ſubſtance was of a greeniſh yellow here and there, and of a blueiſh grey where it touched the crucible; being broken it produced a black grey powder. It was mixed with a dram of ſulphuric acid diluted with five drams of water; at that inſtant there was a rapid diſengagement of gas, and on heating the mixture it evidently gave out hydrogen gas. After a ſuffi-

cient

The [illegible] oxide of [illegible] mixed with [illegible] of

cient ebullition, water was added, and the ſolution decanted off clear. The reſidue was treated again with weak ſulphuric acid, and then gave a reſidue of ten grains which was by no means oxide of cobalt, but oxide of nickel mixed with cobalt, as its ſolutions in the acids and in ammonia proved. The two preceeding ſolutions were each ſeparately analyſed by pure potaſh, and the precipitate was beſides heated with an exceſs of potaſh, and then waſhed and dried. At the proof each of the precipitates afforded cobalt, which was always moſt pure in that of the firſt ſolution; for the ſolution in muriatic acid, laid on paper, and heated, inclined perceptibly to a yellow, while the precipitate of the ſecond ſolution produced a ſtain of a clear and pure green. It is ſtrange that the firſt ſolution afforded more oxi-muriatic acid than the ſecond.

E[illegible] of the pre[illegible]tes of the [illegible]ing ſolutions afford cobalt.

Theſe experiments, and others made by the author; but not related, prove,

The experiments prove that nickel oxide does not become yellow; cauſe of the miſtake of Dr. [illegible]chnaubert on [illegible] point.

A. That the oxide of nickel heated either ſlightly or violently does not aſſume a yellow colour; and if this colour was obſerved by M. Schnaubert, it muſt have been cauſed by ſome ſubſtances which entered into the compoſition of the oxide, or perhaps by the mixture of a little arſenic.

It is [illegible] poſſible to o[illegible] pure nick[illegible] [illegible]his way

B. That it is impoſſible by M. Schnaubert's method, to obtain an oxide of nickel exempt from cobalt; ſince it does not even effect a ſeparation of the two oxides ſo far as to be perceptible to the eye.

M. [illegible]man's method too trouble[illegible] and ex[illegible], and M. [illegible]gman's alſo

C. M. Bucholz hints here at ſeveral experiments he made with a view to find an acid which would form an inſoluble ſalt with one of the oxides, and one eaſy of ſolution with the other, but which, as they did not ſucceed, he does not mention; and as the method propoſed by Mr. Lehman (in the *Cadmiologia*, part II, page 110) of fuſing fifteen or twenty times, to a commencement of vitrification, a mixture of nickel and cobalt, in order to ſcorify all the cobalt, would be too troubleſome and expenſive, as would that alſo indicated by Bergman (*Opuſcul, Phyſic, et chem,* Vol. II. p. 246—249) of repeating the fuſion three or four times with from 8 to 12 times the quantity of pure nitre. The proceſs indicated A 4, (conſiſting of a partial decompoſition of the ammonical nitrate of nickel), alone remained to be repeated. For this purpoſe

the [illegible] A 4 repeated.

oxide of nickel, (which was ſeparated from the triple ſalt, not diſſolved at the firſt evaporation, by carbonate of potaſh, was treated repeatedly, (in ſuch a manner) that after diſſolving it in nitric acid, recourſe was had to the uſe of ammonia and evaporation as before deſcribed. In this method was obtained, entirely free from cobalt, an oxide ſeparated by potaſh from the triple ſalt, which had been rediſſolved after evaporation, and which oxide had the properties mentioned in the memoir printed in the ſecond volume of the Annales de Chimie.

The oxide which was ſeparated by evaporation from the ammoniacal nitrate of nickel, was in the laſt operation entirely freed from cobalt; it only contained a ſtill, as has been obſerved, a little nitric acid. The oxide of nickel, which, after having been laid bare by evaporation, ſtill contains cobalt, may naturally undergo the ſame operation over again.

The oxide entirely freed from cobalt in the laſt operation.

This method may be made uſe of untill one more expeditions is diſcovered by farther experiments, ſince it does not occaſion any conſiderable expence, for by potaſh, the evaporation of the ammonical nitrate of nickel may be effected in a retort, and alſo the ſubſequent decompoſition of the triple ſalt, and thus the ammonia may be ſeparated for other uſes; in like manner, in works on a great ſcale, a part of the nitre may be recovered from the laſt operation, by the evaporation of the water in which the ſubſtance has been waſhed.

This method recommended for the preſent, the ammonia may be ſaved during the proceſs.

and the nitre recovered.

XVIII

Sugar prepared from Beets. By M. HERMBSTADT.*

THE method of M. Achard for extracting ſugar from beets, was ſo expenſive, that it was of no advantage for common uſe. M. Hermbſtadt, of Berlin, has practiſed another method, which is eaſily performed, and affords hopes of rendering this ſugar cheaper than that from the ſugar-cane; which is as follows:

After having bruiſed the beets in a mortar, M. Hermbſtadt ſubmits them to the operation of a preſs, to extract the juice from them; which is then placed in veſſels, and clarified with lime in the ſame manner as cane-ſugar.

The expreſſed juice of the beet is clarified by lime, and then evaporated to a ſyrup. A coarſe ſugar is produced on cooling.

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* Sonnini's Journal, Tom. II. p. 331.

When this operation is finiſhed, the liquor is evaporated to the conſiſtence of a ſyrup: It is then left to cool, and a courſe ſugar is obtained, of a dark-brown colour: At the bottom of the veſſel a ſyrup remains, which may be uſed for domeſtic purpoſes.

From 100 lb. of this coarſe ſugar, 80 lb. of refined ſugar may be obtained.

From 100 pounds of the coarſe ſugar, eighty pounds of well cryſtallized ſugar are obtained by the firſt refining, which ſugar is not at all inferior in quality or whiteneſs to that of the cane. The whole operation may be completed in two days.

It is probable M. Hermbſtadt uſed the common field beet, or root of ſcarcity.

The particular ſpecies of beet which M. Hermbſtadt uſed in his experiments, is not mentioned; but it is moſt probable that this chemiſt made uſe of the common field beet, known in Germany by the name of *mangel wortzel*, the culture of which is ſpread through many cantons of Germany. This variety, however, contains leſs ſugar than all the other ſpecies of beets; and, for this reaſon, M. Sonnini is of opinion, that if ſugar can be obtained from beets with profit and economy, more ſucceſs would be obtained by ſubmitting to the operations deſcribed, the ſmall red beet, called in France that of *Caſtlenaudery*, which is the ſweeteſt of all.

A better produce might be obtained from the ſmall red beet of Caſtlenaudery.

XIX.

Method of ſtacking Turnips, to preſerve them through the Winter. By Mr. JOHN SHIRREFF, *of Captain Head, near Haddington, N. Britain.**

Rapa ſolo molli et aere humidulo lætantur.

Preſervation of turnips through the winter.

SATISFIED, from obſervation and experience, that turnips are the foundation of the beſt huſbandry on almoſt all ſoils and ſituations in the arable diſtricts of Great Britain; and that this crop ſhould always be drawn, except from blowing ſands, or light mooriſh ſoil, on both of which it ſhould always be in part conſumed on the ground with ſheep; convinced alſo, that turnips, if poſſible, ſhould be off all ſoils, and the land

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* Soc. Arts, 1805. The premium of 30 guineas was awarded for this method.

Preſervation of turnips through the winter.

ploughed up before the middle of December, at the lateſt, to ſecure the ſucceeding corn crop, and graſſes, or clovers, with either of which every field that carried a turnip crop the preceding ſeaſon, ſhould, in almoſt every caſe, be ſown down; and impreſſed with the many high advantages attending this practice, as ſoon as my pea and bean ſtubbles are ploughed up, and ſown with wheat, my turnips are begun to be drawn, and ſtacked up for uſe during the following winter and ſpring. If the diſtance of the turnip-field from the homeſtead does not exceed a quarter of a mile, two double horſe carts only are employed, and more in proportion to the diſtance of the turnip field, or number of hands you may be able to command to carry on the work. One clever driver is ſufficient for two carts, and two for three carts, &c. one cart being always in the field loading or loaded. On being brought home, the turnips are inſtantly tumbled out at the ſtack; which is done with great facility, from the conſtruction of the carts in this diſtrict, which to convenience and ſtrength likewiſe add lightneſs, to enable horſes to move at a ſmart pace with them when empty. The turnips tumbled out of the cart, are trimmed of their leaves, and cleaned of any earth that may adhere to them, by women, &c. before being put into the ſtack. Old table-knives do very well for the purpoſe, and the leaves ſhould be cut off cloſe to the root; the back of the knife being uſed for removing any pieces of ſoil that may ſtick on the turnip.

Women, &c. trim the turnips, and put them into ſtrong coarſe wicker baſkets, to be carried forward by a man, who hands them to another, who lays them into or on the ſtack. The ground on which the turnips are placed ought to be dry bottomed. If that is not the ſort of ſoil where you find it moſt convenient to make your ſtack, a quantity of boulders may be put on, regularly ſpread over the ſpace, to the thickneſs of at leaſt eighteen inches. My corn-rick yard, being dry ground, has been uſed as the place for keeping my turnips in. The ſtacks have been made about ten feet wide, by driving a row of ſtakes into the ground parallel to the wall of the yard, which ſerves inſtead of another row. The wall is only about five feet and a half high, and the ſtakes are driven to the ſame height. The inſide of the wall and ſtakes are lined with compact bunches, or ſheaves of wheat-

Preſervation of turnips through the winter.

ſtraw, about ten inches in diameter, placed horizontally on the ground or boulders, and introduced, as wanted, during the operation of ſtacking. A tire of the largeſt turnips are placed one above another, on the inſide of the bundles of ſtraw, more particularly on the ſide guarded by the ſtakes, till the pile reaches the height of five feet from the ground, or from the boulders, if it has been found neceſſary to ſpread any over the ground. The inner part of the ſtack is at the ſame time gradually made up with turnips put in promiſcuouſly; along which a plank is laid, and occaſionally ſhifted as the pile riſes, for the man who builds the ſtack to ſtand on without bruiſing the turnips with his ſhoes. When the pile of turnips is reared, in the manner deſcribed, to the height of above five feet, it is gradually contracted inwards, on both ſides, at an angle of about forty-five degrees, like the roof of a barn; the largeſt turnips being ſtill piled on the outſide, till the roof is ſo far completed. The ſtack is every day ſo far finiſhed in height as it is extended in length, and is covered with wheat ſtraw thatch, roped down with twiſted bands of oat-ſtraw before evening, to ſecure the ſtacked turnips from rain that may fall during the night. The thatch is laid on a foot thick, and ſecured in the ſame ſimple, effectual manner, that corn-ricks are covered in Northumberland, Berwickſhire, and the Lothians; with this difference only, that the ſtraw is four times as thick laid on the turnip as on the corn, to exclude cold as well as wet; and that there is a rail of wood ſtretched, hanging horizontally at the tops of the wall and ſtakes, to fix the ſtraw ropes to, which ſecure the thatch on the ſtack. The end of the ſtack is every night covered with bundles of wheat-ſtraw, which are removed next day, or when building recommences.

Three men are employed in the field to load and diſpatch the carts, occaſionally aſſiſting four women who draw the turnips, ſtriking off the top root with a ſtrong heavy knife, leaving the turnips on the tops of the drills as drawn and chopped, with the leaves all in one direction, to be readily laid hold of by the men who lift them up to the cart. The horſes paſs along in the ſpace between the two rows or drills of the turnips, which may be drawn: and, being at thirty inches apart, and the extremities of the wheels about five feet from each other, it is evident a wheel runs in the middle

of

Preſervation of turnips through the winter.

of each ſpace between the contiguous drills, without injuring the turnip, whether drawn or not. When the cart is about to turn, after being loaded, the men move the turnips to make room for the horſes, putting them into the cart as part of the load.

Expences of drawing, carting, trimming, ſtacking, covering, &c. a ſtatute acre of good turnip,—at the diſtance of not more than a quarter of a mile from the ſtack.

	£.	s.	d.
Two double-horſe carts, and one man -	0	16	0
Two men loading, drawing, building, &c. -	0	8	4
Seven women drawing and trimming - -	0	4	1
Two girls trimming - - - - - -	0	1	0
Four ditto and boys ditto - - - - -	0	1	8
Twiſting ropes, drawing thatch, thatching, waſte of thatch, ſtakes, &c. ſay - - -	0	3	6
	1	14	7

The above is a fair average of the expence of ſecuring ſomewhat more than twelve and a quarter ſtatute acres laſt ſeaſon, which was all I drew; and one field of two acres, one rood, thirty-three perches, was ſo far diſtant as to require three carts, and two drivers. That field, however, was firſt drawn, and the weather being fine and moderate, more work was done in proportion to the length of the day, which was alſo longer. Women and children cannot, indeed, exert themſelves with ſpirit, in raw cold weather. October is perhaps the beſt month to draw in. It is a queſtion with me, whether the average of the acres that are under turnip in the iſland, if the weight exceeds twenty-four tons, does not coſt more, merely for drawing and carting only. When it is conſidered that this operation is performed often in cold, froſty, and ſtormy weather, and that frequently much ſnow may be to be removed before the turnip can be ſeen. If no ſnow has fallen before the froſt ſets in, the turnips muſt be hoed up with inſtruments for the purpoſe. Many are cut, and much left in the ground of the lower part of the root. After all this labour, what is obtained is frequently no better than a lump of ice, environed with earth, frozen ſo firmly to its ſurface, that nothing but thawing in

Preſervation of turnips through the winter.

cold water can ever render it fit to be touched by the mouth of any animal whatever.

Admitting, however, the expence of drawing and carting to be the ſame, all that can be ſtated as extraordinary expence is the coſt of trimming and ſacking, which amounts to 11s. 3d. an acre. On the other hand, we have the advantage of having fine freſh clean turnips, always ſecure and at command, to carry on feeding and breeding ſtock; at the ſame time that all loſs by rotting in the ſpring months is prevented; which is frequently thirty and even fifty per cent. on all the crop that remains in the field, after the firſt of February. Above all, the practice of drawing and ſtacking before winter, by admitting of early ploughing to mellow the ſoil, ſecures a valuable corn, and ſucceeding clover crops. When all theſe circumſtances are maturely weighed, the expence of eleven ſhillings and three-pence will, to every enlightened agriculturiſt, appear but trifling to obtain ſuch very valuable advantages. The writer of this little eſſay has had the ſatisfaction of having excellent crops after his turnips, this ſeaſon; while almoſt every other crop in the neighbourhood was indifferent; and ſome on rich dry loams, high rented, by being ſown in the months of April and May, on the ſpring ploughing, after turnips eaten off with ſheep, were ſo miſerable, as evidently to pay *nothing* after expences of labour, ſeed, and reaping. The young clovers too, ſown with theſe crops, have almoſt entirely periſhed from want of moiſture. The loſs of the crop and clover ſeed is not all: the ſyſtem ſuffers a derangement, the conſequences of which none but practical men can calculate.

One thing remains to be noticed, which is, that twenty-ſix young cattle, cows, and yearling calves, were kept nearly three weeks on the turnip trimmings, with oat-ſtraw along with them, to their improvement; and that many more might have been kept, had they been provided in time. A quantity of good manure was made: and, eſtimating all advantages ariſing from the conſumption of the leaves in this way, at no more than 3d. a head per night, for the keep of each beaſt, the amount will exceed the expence of trimming and ſtacking the whole crop of turnips on twelve acres and a quarter.—The leaves that remain on turnips after Chriſtmas, are either unfit to be eaten, or waſted by the froſts.

T. SHIRREFF.

XX.

Account of ſome Specimens of Baſaltes from the northern Coaſt of Antrim. *By the Rev. Dr.* WILLIAM RICHARDSON.*

Remarks on the baſaltes of the coaſt of Antrim.

THE Reverend Dr. William Richardſon, late F. T. C. D. having ſent to Dr. Hope a collection of ſpecimens from the northern coaſt of Antrim, with a catalogue and obſervations, the ſpecimens were exhibited; and the obſervations were read in the Royal Society, March 1803.

Siliceous Baſalt.

Dr. Richardſon diſcovered the foſſil to which he gives this name, in the peninſula of Portruſh, four or five years ago. It abounds alſo in the Skerry iſlands, a reef of rocky iſlots extending from the northern point of Portruſh-head for about a mile eaſtward. A ſmall part of every one of thoſe iſlots is formed of this ſtone, while the remainder conſiſts of coarſe baſalt, ſimilar in all reſpects to that on the eaſt ſide of the above-mentioned peninſula. It is met with in one or two other places.

This ſtone is arranged in ſtrata, from ten to twenty inches thick, all ſteadily parallel to one another, and every ſtratum, as far as can be obſerved, preſerving an uniform thickneſs through its whole extent. When theſe ſtrata are quarried into, they appear to be conſtructed of large priſms, generally pentagonal, which when broken divide into ſmaller priſms. This internal priſmatic conſtruction frequently gives an irregular or ſhivery appearance to the fracture, which however is often conchoidal, and the grain as uniform as in the Giant's Cauſeway baſaltes.

The beds of this foſſil are remarkable for containing marine exuviæ in great abundance, particularly impreſſions of *cornua ammonis*. The flat ſhells and impreſſions contained in theſe ſtones, are ſteadily parallel to each other, and perpendicular to the axis of the priſms. It muſt be obſerved, that the priſmatic conſtruction is never interrupted by the ſhells diſperſed through it; the planes which ſeparate the priſms paſſing equally through the ſhells and the ſtone itſelf.

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* Edinburgh Tranſ. Vol. V.

Remarks on the baſaltes of the coaſt of An-

The grain of this ſtone paſſes by inſenſible ſhades from a high degree of fineneſs, until it become undiſtinguiſhable from that of the common columnar baſaltes.

The name of Siliceous Baſalt, which Dr. Richardſon employs, was firſt given to this foſſil by Mr. Pictet of Geneva, when he viſited Portruſh, in a tour through Ireland two years ago. He conſidered it as a variety of baſalt, containing a greater proportion of *ſilica* than uſual.

The ſtrata of ſiliceous baſalt, both at Portruſh and the Skerry iſlands, generally alternate with ſtrata of equal thickneſs of a coarſe-grained baſalt of a grey colour. The materials of the ſtrata grow into each other, ſo as to form one ſolid maſs, from which it is eaſy to quarry pieces in the confine of the two ſtrata, with a part of each adhering; but the coarſe baſalt, as it approaches very near to the fine, always abates ſomewhat of its coarſeneſs; yet the line of demarcation is left completely diſtinct.

(The concluſion in our next.)

SCIENTIFIC NEWS.

Almanack printed at Conſtantinople.

Almanack printed at Conſtantinople.

FOR the firſt time an almanack has been printed at Conſtantinople, under the direction of Abdorahman. The printing-office was eſtabliſhed in 1716, by *Said* (who had been at Paris with his father, the ambaſſador), and by Ibrahim, an Hungarian: Achmet the Third patronized them, and they printed many books; but an almanack was never before printed.

Obſervatory at Bavaria.

Bavarian obſervatory.

The Elector of Bavaria, a few months before the arrival of the French armies, cauſed an obſervatory to be erected in the neighbourhood of Munich. The ſituation choſen for its conſtruction, takes in an extenſive horizon. Profeſſor Seyfer, a celebrated aſtronomer of Gottingen, was nominated director of this eſtabliſhment.

Eſtabliſhments for Natural Philoſophy in the Ukraine.

Eſtabliſhment for natural philoſophy in the Ukraine.

The rich land-owners in the Ukraine and Volhinia, have contributed largely for the eſtabliſhment of Lyceums for teaching natural philoſophy, at Krzeminico, and at Winnica. The library and philoſophical apparatus of the King of Poland, have been purchaſed for this purpoſe. M. Sniadecki has received a ſum equal to 500*l.* to purchaſe teleſcopes and clocks; and no expence is to be ſpared in properly furniſhing the obſervatories with inſtruments.

Obſervatory at Moſkow.

Obſervatory at Moſkow.

M. Goldbach, an able aſtronomer of Leipſic, has been nominated profeſſor of the univerſity of Moſkow, with a ſalary equal to 250*l.* He is to have the direction of the conſtruction of a new obſervatory, to furniſh it with inſtruments, to make regular obſervations, and to inſtruct ſome young men in practical aſtronomy who have been previouſly inſtructed in the preparatory ſciences, and to give a courſe of lectures in theoretical aſtronomy in one of the halls of the Univerſity.

They poſſeſs many of Cary's teleſcopes, of different powers; an excellent aſtronomical clock; a chronometer, made by Arnold; a portable circle, of one foot diameter; and, it was reported, had ordered one of three feet diameter from the ſucceſſor of Mr. Ramſden: Thus M. Goldbach will be provided with every inſtrument neceſſary and uſeful to aſtronomy, at the obſervatory of Moſkow.

M. Goldbach has taken the opportunity of his journey, to determine the poſition of ſome towns; among others that of Riga, 1^h 27′.0, and 56° 57′.8.

At the ſame time that M. Goldbach is engaged with the aſtronomical eſtabliſhment at Moſkow, MM. Schubert and Wiſniewſki are employed at the obſervatory of Peterſburgh; and there is reaſon to expect a ſeries of obſervations from that part of the world.

Solar Tables.

Solar Tables.

A ſet of tables of the ſun, compoſed by M. Delambre, have been printed at Paris, in which there are many new equations, and of which all the elementary parts have been verified by new obſervations. A ſet of tables of the moon's motion are alſo to be printed, and when they are completed, thoſe of the planets will follow.

Bequeſt of Erneſt the Second relative to his Obſervatory.

Erneſt the Second's bequeſt to his obſervatory.

Erneſt the Second, late Duke of Saxe-Gotha, was remarkably attached to aſtronomical ſtudies. He made obſervations and calculations himſelf, aſſiſted in compoſing books on the ſubject, and furniſhed the funds for their publication. He enabled M. Zach to meaſure a degree of the meridian in Germany, and defrayed the expences from his private purſe; ſo that he united to the merit of a connoiſſeur in the ſcience, that of an author, a patron, a man of ſcience, and of a generous prince.

He left in his will a ſum equal to about 1330*l.* to form a fund for the maintenance of the obſervatory of Seeberg, near Gotha, which was built out of his own private eſtate; and ordered his ſucceſſor to erect no other monument to his fame, but the careful ſupport of this eſtabliſhment.

Baron de Zach, who has given a copy of the will in his Journal, adds, "That he can aſſure the lovers of ſcience, that the will of the father will not only be fulfilled, but ſurpaſſed by his ſucceſſor, the preſent Duke Emilius Leopold Auguſtus, who has already ſhewn the moſt marked proofs of his attachment to the ſciences.

"In a codicil to the will the Duke repeated, 'I forbid expreſsly the elevation of any monument to my memory, or even an epitaph, or any monument at or near my tomb.'"

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1806.

ARTICLE I.

Letter from T. YOUNG, *M.D. F.R.S. &c. claiming the Lamp deſcribed in our laſt Number, and demanding an Explanation from the anonymous Communicator.*

To Mr. NICHOLSON.

SIR,

Concerning the invention of a Lamp.

I WAS much ſurprized on ſeeing, a few days ago, the figure of a lamp contained in the fourth plate of your Number for February laſt. I truſt you will be convinced, upon inſpection of the figure which I now ſend you, and which was engraved before Chriſtmas, that your correſpondent A. F. muſt have copied his lamp from that which is here repreſented; and I am ſure you will think I have a right to demand a public explanation of the manner in which he procured a ſight of a plate not yet publiſhed, and of the motives which induced him to make ſo unjuſtifiable a uſe of it. I ſhall reſerve the complete explanation of this lamp for the work to which the plate belongs, which has been long in the preſs, and which will ſoon be ready for publication; I ſhall only obſerve that it is in a great meaſure free from the inconvenience which A. F. has attributed to it, (p. 168) and that the "ſmall ſhaded circle" is not a "perforation," but a weight attached to the counterpoiſe,

I am, Sir,
Your very obedient Servant,
THOMAS YOUNG.

Welbec Street,
March 15, 1806.

II.

On the Tendency of Elaſtic Fluids to Diffuſion through each other. By JOHN DALTON [*].

Mixed elaſtic fluids of different denſities do not ſeparate;

IN an early period of pneumatic chemiſtry it was diſcovered that elaſtic fluids of different ſpecific gravities being once diffuſed through each other, do not of themſelves ſeparate, by long ſtanding, in ſuch manner as that the heavieſt is found in the loweſt place; but on the contrary, remain in a ſtate of uniform and equal diffuſion.

but will they mix without agitation.

Dr. Prieſtley has given us a ſection on this ſubject (vid. Experiments and Obſervations, &c. abridged. Vol. II. p. 441) in which he has proved the fact above-mentioned in a ſatisfactory manner; and every one's experience ſince, as far as I know, has coincided with his concluſions. He has not offered any conjecture concerning the cauſe of this deviation from the law obſerved by inelaſtic fluids; but he ſuggeſts that

Dr. Prieſtley thinks not.

"if two kinds of air of very different ſpecific gravities, were put into the ſame veſſel, with very great care, without the leaſt agitation that might mix or blend them together, they might continue ſeparate, as with the ſame care *wine* and *water* may be made to do."

The determination of this point, which ſeems at firſt view but a trivial one, is of conſiderable importance; as from it we may obtain a ſtriking trait, either of the agreement or diſagreement of elaſtic and inelaſtic fluids in their mutual action on each other.

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[*] Mancheſter Memoirs, Vol. I. New Series.

Inquiry by experiment which ſhews the contrary.

It is, therefore, the ſubject of the following experiments to aſcertain whether two elaſtic fluids brought into contact, could intermix with each other, independently of agitation. The reſult ſeems to give it in the affirmative beyond a doubt, contrary to the ſuggeſtion of Dr. Prieſtley; and eſtabliſhes this remarkable fact, *that a lighter elaſtic fluid cannot reſt upon a heavier*, as is the caſe with liquids; but, they are conſtantly active in diffuſing themſelves through each other till an equilibrium is effected, and that without any regard to their ſpecific gravity, except ſo far as it accelerates or retards the effect, according to circumſtances.

Apparatus.

The only apparatus found neceſſary was a few phials, and tubes with perforated corks; the tube moſtly uſed was one ten inches long, and of $\frac{1}{20}$ inch bore; in ſome caſes a tube of 30 inches in length and $\frac{1}{3}$ inch bore was uſed; the phials held the gaſes that were ſubjects of experiment and the tube formed the connection. In all caſes, the heavier gas was in the *under* phial, and the two were placed in a perpendicular poſition, and ſuffered to remain ſo during the experiment in a ſtate of reſt; thus circumſtanced it is evident that the effect of agitation was ſufficiently guarded againſt; for, a tube almoſt capillary and ten inches long, could not be inſtrumental in progagating an intermixture from a momentary commotion at the commencement of each experiment.

FIRST CLASS.

Carbonic Acid Gas, with Atmoſpheric Air, Hydrogenous, Azotic and Nitrous Gaſes.

Carbonic acid gas with lighter gaſes.

1. A pint phial filled with carbonic acid gas, the 30 inch tube and an ounce phial, the tube and ſmall vial being filled with common air, were uſed at firſt. In one hour the ſmall phial was removed, and had acquired no ſenſible quantity of acid gas, as appeared from agitating lime water in it. In three hours it had the acid gas in great plenty, inſtantly making lime water milky. After this it was repeatedly removed in the ſpace of half an hour, and never failed to exhibit ſigns of the acid gas. Things remaining juſt the ſame, the upper phial was filled with the different gaſes mentioned above repeatedly, and in half an hour there was always found acid ſufficient to make the phial $\frac{1}{2}$ filled with lime water quite

milky. There was not any perceptible difference whatever gas was in the upper phial *.

SECOND CLASS.

Hydrogenous Gas with Atmoſpheric Air and Oxigenous Gas.

Hydrogen, with atmoſpheric air and oxigen.

1. Two ſix ounce phials were connected by the tube of a tobacco pipe, three inches long, the upper containing hydrogenous gas, the lower atmoſpheric air: after ſtanding two hours, the lower phial was examined; the mixed gaſes it contained made ſix exploſions in a ſmall phial. The gas in the upper alſo exploded.

2. Two four ounce phials connected with the ten inch ſmall tube ſtood two days, having common air and hydrogen gas. Upon examination the upper was found to be $\frac{1}{3}$ common air by the teſt of nitrous gas. The gas in the under exploded ſmartly; that in the upper moderately with a lambent flame.

3. Two one ounce phials were connected by the ten inch tube, containing common air and hydrogenous gas; in three hours and a half the upper was about $\frac{1}{3}$ common air and the under $\frac{2}{3}$; the former exploded faintly; the latter ſmartly.

4. Two one ounce phials were connected as above; the under containing gas about $\frac{3}{4}$ oxygenous, the upper hydrogenous: In three hours the latter was $\frac{1}{5}$ oxygenous, and the former about $\frac{1}{2}$; the upper exploded violently, the under, moderately.

5. Two one ounce phials were again connected, the lower having atmoſpheric air, the upper hydrogenous gas; they ſtood fifteen hours, and were then examined; the upper gave 1.67 with nitrous gas, the under 1.66.—Hence it is evident that an equilibrium had taken place, or the two gaſes were uniformly diffuſed through each other in both phials.

THIRD CLASS.

Nitrous Gas, with Oxigenous Gas, Atmoſpheric Air, Hydrogenous and Azotic Gaſes.

Nitrous gas with oxigenous, atmoſpheric hydrogenous, and azotic.

The reſults of the preceding experiments upon gaſes that have no known affinity for each other, were conformable to what

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* The ſmall tube of ten inches was then uſed and a phial of common air; in one hour much acid gas had come through, as appeared by lime water.

Nitrous gas with oxygenous, atmoſpheric hydrogenous, and azote.

what *à priori*, I had conceived; for, according to my hypotheſis, every gas diffuſes itſelf equably through any given ſpace that may be aſſigned to it, and no other gas being in its way can *prevent*, though it may conſiderably *retard* this diffuſion. But in ſome of the following experiments, in which the two gaſes are known to have a chemical affinity for each other, I expected different reſults from what are found; perhaps without ſufficient reaſon. For, chemical union cannot take place till the particles are brought into contiguity; and the elaſtic force which ſets them in motion appears, from the above experiments, to be a principle diametrically oppoſite to affinity. That circulation of elaſtic fluids, therefore, which we have now before us, cannot be *accelerated* by their having a chemical affinity for each other. Another circumſtance deſerves explanation;—when nitrous and oxygenous gas are in the two phials, the reſiduary gaſes after the experiment are nearly as pure as before; becauſe thoſe portions of them that meet in the tube, form nitrous acid vapour, which is abſorbed by the moiſture in the phials, and therefore does not contaminate either gas.

1. Two one ounce phials were connected with the ſmall tube, the under containing nitrous gas, the upper atmoſpheric air; after three hours, the upper phial was taken off when a quantity of air was perceived to enter, as was expected; the air in the upper phial was ſcarcely diſtinguiſhable from what it was at firſt; that in the under phial was ſtill ſo much nitrous as to require its own bulk of common air to ſaturate it.

2. The above experiment was repeated, and the upper phial drawn off when the whole was under water, in order to prevent communication with the atmoſphere: about $\frac{1}{6}$ of an ounce of water entered the phials, to compenſate the diminution. Remaining air in the upper phial was a very little worſe than common air, it being of the ſtandard 1,47 when the former was 1,44. The gas in the under phial was ſtill nitrous and nearly of the ſame purity as at firſt; for three parts of it required four of atmoſpheric air to ſaturate them.

3. Nitrous gas and one $\frac{2}{3}$ oxygenous were tried in the ſame way: after four hours, the apparatus was taken down under water. The upper phial was $\frac{2}{3}$ filled with water, and the

gas in it was partly driven down the tube into the other phial, by which, and the previous proceſs, the nitrous gas was completely ſaturated and nothing but azotic with a ſmall portion of oxigenous were found in the under phial: the remaining gas in the upper phial was ſtill $\frac{1}{2}$ oxygenous.

4. Nitrous gas and hydrogenous: in three hours the upper phial was $\frac{1}{4}$ nitrous, and of courſe the under muſt have a like part of hydrogen.

5. Nitrous gas and azotic: after three hours the upper phial was $\frac{1}{3}$ nitrous.

In the two laſt experiments, the quantity of nitrous gas in the upper phial was leſs than might be expected; but the tube was at firſt filled with common air, and ſome muſt enter on connecting the apparatus, which is ſufficient to account for the reſults.

FOURTH CLASS.

Azotic Gas, with Mixtures containing Oxigenous Gas.

Azote with oxygenous compounds.

1. Azotic gas and one $\frac{2}{3}$ oxygenous: after ſtanding three hours the upper phial was of the ſtandard 1.78, or about $\frac{1}{14}$ oxygenous.

2. Azotic gas with atmoſpheric air: after ſtanding three hours: the upper phial was not ſenſibly diminiſhed by nitrous gas; the under phial, however, had loſt two per cent, or $\frac{1}{10}$ of its oxigen. The reaſon of this was, that the azotic gas in this experiment having been juſt made for it from nitrous gas, this laſt had not been completely ſaturated with atmoſpheric air, and hence had ſeized upon all the oxygen aſcending into the upper phial.

Having now related all the experiments I made of any importance to the ſubject, it will be proper to add, for the ſake of thoſe that may wiſh to repeat ſome of them, that great care muſt be taken to keep the inſide of the tube dry; for if a drop of water interpoſe between the two gaſes, I have found that it effectually prevents the intercourſe: glaſs tubes ſhould therefore be uſed, that one may be ſatisfied on this head, as the obſtruction will then be viſible.

I ſhall make no further comments on the above experiments, by way of explanation: becauſe to thoſe who underſtand my hypotheſis of elaſtic fluids, they need none: and I think it would be in vain to attempt an explanation any other way.

The remarkable experiment of Prieſtley, of air entering earthen retorts while water paſſed out in a vapor.

I cannot however, on this occaſion, avoid adverting to ſome experiments of Dr. Prieſtley, which few modern philoſophers can be unacquainted with: I mean thoſe relating to the ſeeming converſion of water into air. (Vid. Philoſ. Tranſact. vol. 73, page 414,—or his Expts. abridged, vol. 2, page 407.) He found that unglazed earthern retorts containing a little moiſture, when heated, admitted the external air to paſs through their pores at the ſame time that aqueous vapour paſſed through the pores the contrary way or outward; and that this laſt circumſtance was *neceſſary* to the air's entrance. The retorts are air-tight, ſo far as that blowing into them diſcovers no pores; but when ſubjected to a greater preſſure, as that of the atmoſphere, or even one much ſhort of it, they are not able to prevent the paſſage of elaſtic fluids. The fact of air paſſing into the retort through its pores, and vapour out of them at the ſame time, are elegantly and moſt convincingly ſhewn by Dr. Prieſtley's experiments, in which he uſed the apparatus repreſented in plate 7, fig. 1, of the edition above referred to. The Doctor confeſſes his explanation of theſe remarkable facts is very inadequate; and no wonder, for it is impoſſible for him or any other to explain them on the commonly received principles of elaſtic fluids. But we will hear what he ſays on the ſubject:—"At preſent it is my opinion, that the agent in this caſe is that principle which we call *attraction* of *coheſion*, or that power by which water is raiſed in capillary tubes. But in what manner it acts in this caſe I am far from being able to explain. Much leſs can I imagine how *air* ſhould paſs one way and *vapour* the other, in the ſame pores, and how the tranſmiſſion of the one ſhould be neceſſary to the tranſmiſſion of the other.—I am ſatisfied, however, that it is by means of ſuch pores as air may be forced through, that this curious proceſs is performed; becauſe the experiment never ſucceeds but in ſuch veſſels as, by the air-pump at leaſt, appear to be porous, though in all ſuch."

Dr. Prieſtley's explanation or conjectures.

The fact is, that the ſteam and air mix by means of the pores.

The truth is, theſe facts ſo difficult to explain are exactly ſimilar to thoſe which are the ſubject of this memoir: only inſtead of a *great number* of pores we have *one* of ſenſible magnitude, (the bore of the tube.) Let the porous retort have the ſame elaſtic fluid within and without, in the one caſe; and the two phials contain the ſame elaſtic fluid in the other, then no tranſmiſſion is obſervable in either; but if the retort have common air, or any other gas, without, and aqueous vapour, or any other elaſtic fluid, except the outſide one, within; then the motion in and out commences, juſt as with the phials in ſimilar circumſtances. In fact this laſt obſervation has ſince been verified by Dr. Prieſtley himſelf, of which an account is given in No. 2, of the American Philoſophical Tranſactions, vol. 5. After alluding to his experiments above-mentioned, he obſerves, "Since that time I have extended and diverſified the experiments, and have obſerved, that what was done by *air* and *water*, will be done by *any two kinds of air*, and whether they have affinity to one another or not, that this takes place in circumſtances of which I was not at all apprized before, and ſuch as experimenters ought to be acquainted with, in order to prevent miſtakes of conſiderable conſequence."

—and the ſame happens in any two gaſes.

The facts ſtated above, taken altogether, appear to me to form as deciſive evidence *for* that of elaſtic fluids which I maintain, and *againſt* the one commonly received, as any phyſical principle which has ever been deemed a ſubject of diſpute, can adduce.

III.

On the Horizontal Moon. By Dr. Okely. *In a Letter from* *Mr.* H. Steinhauer.

To Mr. NICHOLSON.

SIR, *Fulnuk, March* 1, 1806.

ABOUT the beginning of laſt year, I had the pleaſure, in compliance with your obliging letter to ſend you impreſſions of the Egyptian Scarabacus, which I hope came ſafe to hand. Your kindneſs in inſerting my trivial remarks upon the ſame in your valuable Journal, encourage me to ſubmit the following ſhort eſſay, upon a ſubject which has employed the ingenuity of ſeveral of your correſpondents, which I received from my friend Dr. Okely, of Wyke, near Hallifax, in conſequence of ſome converſations occaſioned by the peruſal of your work.

If you think it worthy a place in your collection, it will be conſidered as an additional obligation conferred on,

SIR,
Your obedient ſervant,
H. STEINHAUER.

Obſervations on the ſeemingly enlarged apparent Diameters of the Sun and Moon, when viewed in or near the Horizon.

General fact ſtated that the heavenly bodies ſeem larger at low altitudes.

Every one who views the ſun or moon, when they are in the horizon, thinks that they appear larger than when they are ſeen in any more elevated part of the heavens. And aſtronomers know, that the diſtance of the ſame fixed ſtars is apparently greater when ſeen near the horizon, than when they are more elevated. But it is likewiſe well known to aſtronomers, that the apparent magnitudes of the ſun and moon, as well as the apparent diſtances of any given fixed ſtars, as meaſured by the micrometer, are the ſame in that part of the heavens which is near the horizon, as, in the ſame circumſtances, they are found to be in any other part, except that the moon, being really perceptibly farther from an obſerver, placed on the earth's ſurface, when ſhe appears in the horizon, than when ſhe appears in the zenith, is found to have a ſmaller apparent magnitude, agreeing with the cauſes to which it is known to be owing. The firſt mentioned phenomena muſt therefore belong to the head of *optical deceptions*. Let us enquire from what ſource this deception ariſes.

I am not the firſt by whom the ſource was ſought for in the apparent flatneſs of the ſky; but I differ as far as I know, from all others in my manner of connecting one appearance with the other.

Explanation of the flattened appearance of the heavens.

In order to explain my idea of the matter, I ſhall firſt attempt to ſhew that the flattened appearance of the viſible heavens is not an illuſion, but a reality; or in other words, that an obſerver placed on the earth is really at a greater diſtance from a point of the ſky, ſituate in the horizon, than from a point ſituate in the zenith.

The ſky is a real object, and offers a flattened concavity.

This will appear in the cleareſt manner if we endeavour to give an anſwer to the two following queſtions: *What is the ſky?* and *Where is the ſky?*

By the ſky, I mean that blue concave ſuperficies, within which every obſerver on the ſurface of the earth finds himſelf placed. What is this? It is certainly ſomething real and material, or elſe it would not appear coloured. For bodies, to appear coloured, muſt have parts of ſome determinate magnitude.

Where doth it exiſt? Not in thoſe immenſely diſtant parts of ſpace, where the heavenly bodies revolve. For if thoſe ſpaces contained any bodies of a determinate magnitude, and conſequently of a determinate denſity, the heavenly bodies could not continue through ages to revolve in the ſame periodic times; their momentum would be diminiſhed by reſiſtance, and the periodic times of their revolution would change. The blue ſky therefore cannot be placed beyond the atmoſphere of our earth. The ſmalleſt parts of bodies, that are coloured are blue, and the blue ſky is therefore either the atmoſphere itſelf or the ſmalleſt and moſt elevated vapours aſcending in it, or both together. The heavenly bodies ſhine through it, and therefore it cannot be opaque; it is itſelf of a blue colour, and therefore is not perfectly tranſparent.

Though we are ignorant of the exact height of the atmoſphere, yet we may take it for granted, that it does not extend as far as the moon, and therefore that the diſtance of its fartheſt points from the centre of the earth has a finite ratio to the ſemi-diameter of the earth. That the ratio is probably leſs than 2 : 1.

If therefore the blue heavens which ſurround the earth, and are concentric with it, have a ſemidiameter not double that of the earth, their horizontal points as viewed from the earth, muſt be farther from us than any that are nearer the zenith.

For let A C F (*Plate* VII. *Fig.* 1.) repreſent a great circle of the earth, and A C be its radius, and let the circle D B E G repreſent a great circle of the atmoſphere drawn with a radius A B not = 2. A C, the line C D 7 B C; H C 7 B C.

—which is not always alike.

I was led to this ſolution of the flattened appearance of the heavens, by obſerving that, when the ſky is uniformly overcaſt with clouds, the concave ſuperficies appears conſiderably flatter than when the ſky is ſerene. In the former caſe, the two concentric circles in the figure approach nearer to each other, the clouds being nearer to the earth than the ſky is, and the ratio of D E to C B muſt of courſe increaſe.

Explanation from a diagram of the ſky, that

But to proceed. When any bodies ſituated behind a ſemitranſparent ſcreen are ſeen through it, they will appear to be

fixed

the heavenly bodies muſt appear largeſt in the horizon.

fixed in the ſcreen at the points of interſection, which lines drawn from every point of the bodies to the eye of the obſerver, make with the ſcreen. Now ſuch a ſemi-tranſparent ſcreen, the blue ſkies interpoſe between the heavenly bodies and our eyes. They will therefore appear to be fixed in the ſky, at the above-mentioned points of interſection.

But if lines D C, H C, I C, B C, be drawn ſo that the angle at C are equal, they may be conſidered as coming from the extreme points of bodies which ſubtend equal angles of viſion, or which have the ſame apparent magnitude. The angle D C H may be conſidered as repreſenting the angle of viſion which the ſun ſubtends at the horizon. The equal angle H C I, the angle ſubtended by the ſame body in a more elevated ſituation. But $DH > HI$. Thus the heavenly bodies muſt appear enlarged in their vertical diameters, when in the horizon; and the ſame may be ſhewn of any other diameter They will therefore appear uniformly enlarged; which was the thing to be explained.

W. OKELY.

IV.

Account of ſome Specimens of Baſaltes from the northern Coaſt of Antrim. By the Rev. Dr. WILLIAM RICHARDSON.

(Concluded from Page 273.)

Remark on the baſaltes of the coaſt of Antrim.

THE peninſula of Portruſh lies about ſix miles to the weſt of the Giant's Cauſeway, and on its eaſtern ſurface alone preſents theſe ſtrata.

In the ſpace of about 700 yards, it exhibits in miniature thoſe changes and interruptions of the ſtrata, which occur on the large ſcale along the northern baſaltic coaſt of Ireland. At the place where it emerges from the ſtrand, there firſt occurs a maſs compoſed of ſtrata of the coarſe and ſiliceous baſalt, placed over each other alternately; this is ſucceeded by an accumulation of regular ſtrata of the coarſe baſalt alone. A ſecond alternation, and a ſecond accumulation of the coarſe-grained ſtrata, come in order, and extend to the well called Tubber Wherry. Here commences an accumulation of many ſtrata of the ſili-

ceous

Remarks on the baſaltes of the coaſt of Antrim.

ceous baſalt alone, which ſtretches along the ſhore for about 100 yards; and then changes into a third alternation, which continues to the little boat-harbour, called *Port-in-too*, near which the ſiliceous baſalt diſappears. Over this ſtretch, notwithſtanding the frequent change in the arrangement of the ſtrata, the thickneſs of each ſtratum, of both ſpecies, remains pretty nearly the ſame, and the poſition of them all ſteadily ſo, viz. with a conſiderable dip to E. N. E.

The weſt ſide of the peninſula, though only about 400 yards diſtant, conſiſts entirely of coarſe baſalt. It ſhows a bolder face, and is formed of rude maſſive pillars, from 60 to 80 feet long.

"I am aware," ſays Dr. Richardſon, "that ſeveral mineralogiſts deny the ſhell-bearing ſtone to be baſalt, while others contend ſtrenuouſly that it is. I will not venture to decide on the queſtion, but muſt remark, that I have never met with it but contiguous to baſalt, and ſo ſolidly united to this laſt, that the continuity of the whole maſs was uninterrupted. The grain of the ſtone graduates, as has been already remarked, into that of the common baſaltes; and the arrangement of it and that of the baſalt, with which it is ſo much mixed at Portruſh and the Skerry iſland, is exactly the ſame; the ſtrata of each ſcarcely differing in thickneſs, and not at all in inclination. The ſtrata of both kinds break into priſms, and the ſurfaces, where acceſſible, exhibit the appearance of cauſeways, differing only in this, that in the ſiliceous baſalt, the pentagon is the prevalent figure, and in the coarſe baſalt, the quadrangle. The fuſibility of both ſtones is alſo nearly the ſame; the ſhells in the ſiliceous baſalt are calcined in the fire, and many more are then diſcovered which had before eſcaped the eye *."

Whinſtone

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* Dr. Richardſon obſerves, that ſome mineralogiſts deny that this foſſil is baſalt. Several of the members preſent when this paper was read, ſome of whom had examined the ſtone in its native place, were of that number. It was remarked, that though certain portions of the ſtrata of this foſſil bore much reſemblance to ſome ſpecies of baſalt, by far the greater part of the maſs bore no reſemblance whatever to any.

It was alſo ſtated, that the ſubſtance of the coarſe-grained, undiſputed baſalt, which lies between the ſtrata of this ſtone, does not contain any veſtiges of marine animals; That veins often iſſue from

Whinſtone Dikes on the Coaſt of Antrim.

Remarks on the baſaltes of the coaſt of Antrim.

Dr. Richardſon deſcribes ſome particulars in the conſtruction of the whinſtone dikes on the coaſt of Antrim, which appear ſingular, and deſerving of attention. Theſe dikes, he ſays, are uniformly formed of large maſſive priſms laid horizontally, which are always diviſible into ſmaller priſms that are likewiſe horizontal. To prevent confuſion, he calls the firſt of theſe *component priſms*, and the ſecond, or ſmaller ones into which the others break, *conſtituent priſms*.

The component priſms are ſometimes of enormous ſize, and in the ſame dike are nearly equal; the conſtituent priſms are ſmall, (the ſides about an inch long), and neatly formed.

The dike which traverſes the Giant's Cauſeway, differs from thoſe on other parts of the coaſt, by having no component priſms. It reſembles a plain wall, of which the parts ſhiver under the hammer into very neat conſtituent priſms. In the dike at Seaport the ſame thing is obſerved; the priſmatic ſtructure does not penetrate two inches from its edge; the whole interior ſeems an amorphous maſs.

The ſpecimens of this latter dike, ſent to Dr. Hope, exhibit its continuity with the adjacent baſaltic rock which it traverſes, and alſo the continuity of the fine baſalt of its edge with the *granular* ſtone which compoſes the middle of the dike.

The dike of *Port-coan* is a very ſolid maſs, compoſed of ſtones apparently round, and imbedded in a baſaltic paſte, or indurated mortar. The round ſtones are formed of concentric ſpheres, like the coats of an onion; they exceed a foot in diameter, and, together with the mortar by which they are united, they form a very compact and highly indurated rock.

Beſides theſe large dikes, Dr. Richardſon remarks, that veins from half an inch to an inch and a half thick, often cut the baſaltic ſtrata on that coaſt in all directions. The materials of theſe veins are never the ſame with the contiguous baſalt,

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from the beds of this real baſalt, and pervade the ſuppoſed ſiliceous ſpecies; ſome of them connecting together the ſeparate beds of the real baſalt; others dying away in ſlender ramifications, as they riſe through the interpoſed ſtratum. In no inſtance is this reverſed: The veins never proceed from what is called the Siliceous Baſalt. It was farther obſerved, that both the fracture and external ſurface of this ſtone exhibit a ſtratified ſtructure, in many Inſtances, which never happens in the true baſaltes.

but

Remarks on the baſaltes of the coaſt of Antrim.

but are generally finer. At Portruſh is a large vein, and near it a ſmaller vein, not an inch thick, which, proceeding from below, terminates in the ſolid rock before it reaches the ſurface.

Miſcellaneous Obſervations.

Some of the ſpecimens in Dr. Richardſon's catalogue are from a quarry in a maſs of baſalt at Ballylugan, two miles ſouth of Portruſh. This baſalt contains ſmall cavities in its interior; many of them full of freſh water, which guſhes out when the ſtone is broken by the hammer, as if it had been in a ſtate of compreſſion. The ſtone is ſo hard, and flies ſo in pieces, that Dr. Richardſon has not been able to collect any of the water for the purpoſe of analyſis.

The face of the quarry in which this variety of the baſalt is found is about 15 feet high, and is cut into a ſtratum, the thickneſs of which is not yet aſcertained. The rock is entirely columnar, the pillars ſomewhat ſmaller than thoſe of the Giant's Cauſeway, leſs perfect, not articulated, ſometimes bent, and variouſly inclined. The ſides and the interior of the pillars are full of cavities. In conſequence of the obſervations of Dr. Hamilton and Mr. Whitehurſt reſpecting the porous texture of the air or bladder holes of the baſaltes of the Cauſeway and its vicinity, Dr. Richardſon has examined a great variety; but in no inſtance, except this of Ballylugan, has he found cavities, in the interior of the baſaltic rocks on this coaſt, though they are frequent on the ſurface expoſed to the air.

The laſt variety of whinſtone enumerated by Dr. Richardſon is the Ochrous, which makes, as he ſays, a conſpicuous figure in the ſtupenduous precipices along the coaſt of Antrim. It is diſpoſed in extenſive ſtrata of every thickneſs, from an inch to twenty-four feet, and varies in colour, from a bright minium to a dull ferruginous brown.

Three remarks are made by Dr. Richardſon, that are undoubtedly of importance, and ſhow that this ſtone is merely baſalt in a certain ſtate of decompoſition.

1. The ochrous ſtrata are extenſive; they remain always parallel to the baſalt ſtrata which they ſeparate; they unite to the baſalt without interrupting its ſolidity; the change from the

one to the other is ſudden, and the lines of demarkation are diſtinct. The ochrous ſtone is never found but contiguous to other baſalt.

2. The ſubſtances imbedded in the ochrous rock, and in baſalts, are exactly the ſame; calcareous ſpar, zeolite, chalcedony, &c.

3. Among the varieties which this rock preſents, there may be found every intermediate ſtage between ſound baſalt and perfect ochre. The change is often partial, beginning with veins and ſlender ramifications.

V.

On the Abſorption of Gaſes by Water and other Liquids. By

John Dalton.*

1. If a quantity of pure water be boiled rapidly for a ſhort time in a veſſel with a narrow aperture, or if it be ſubjected to the air-pump, the air exhauſted from the receiver containing the water, and then be briſkly agitated for ſome time, very nearly the whole of any gas the water may contain, will be extricated from it.

Air or gas is extricated from water by boiling and agitation in vacuo.

2. If a quantity of water thus freed from air be agitated in any kind of gas, not chemically uniting with water, it will abſorb its bulk of the gas, or otherwiſe a part of it equal to ſome one of the following fractions, namely, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c. theſe being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c. or $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, $\frac{1}{4^3}$, &c. the ſame gas always being abſorbed in the ſame proportion, as exibited in the following table:—It muſt be underſtood that the quantity of gas is to be meaſured *at the preſſure and temperature with which the impregnation is effected.*

The volume of every gas abſorbed by water is conſtant, and is either equal to the bulk or to the cube of a reciprocal of that bulk,

—equal preſſures and temperatures being ſuppoſed.

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* Manchester Mem. N. S. Vol. I.

Table of quantities.

Bulk abſorbed, the bulk of water being unity. $\frac{1}{1^3} = 1$	Carbonic acid gas, ſulphuretted hydrogen, nitrous oxide.*
$\frac{1}{2^3} = \frac{1}{8}$	Olefiant gas, of the Dutch chemiſts.
$\frac{1}{3^3} = \frac{1}{27}$	Oxygenous gas, nitrous gas,† carburretted hydrogen gas, from ſtagnant water.
$\frac{1}{4^3} = \frac{1}{64}$	Azotic gas, hydrogenous gas, carbonic oxide.
$\frac{1}{5^3} = \frac{1}{125}$	None diſcovered.

3. The gas thus abſorbed may be recovered from the water the ſame in quantity and quality as it entered, by the means pointed out in the firſt article.

Water abſorbs any gas in the ſame quantity, whether it contain another gas or not.

4. If a quantity of water free from air be agitated with a mixture of two or more gaſes (ſuch as atmoſpheaic air) the water will abſorb portions of each gas the ſame as if they were preſented to it ſeparately in their proper denſity.

Ex. gr. Atmoſpheric air, conſiſting of 79 parts azotic gas, and 21 parts oxygenous gas, *per cent.*

Water abſorbs $\frac{1}{64}$ of $\frac{79}{100}$, azotic gas = 1.234
——— $\frac{1}{27}$ of $\frac{21}{100}$, oxygen gas = .778

Sum, *per cent.* 2.012

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* According to Mr. William Henry's experiments, water does not imbibe quite its bulk of nitrous oxide; in one or two inſtances with me it has come very near it: The apparent deviation of this gas, may be owing to the difficulty of aſcertaining the exact degree of its impurity.

† About $\frac{1}{20}$ of nitrous gas is uſually abſorbed; and $\frac{1}{27}$ is recoverable: This difference is owing to the reſiduum of oxygen in the water, each meaſure of which takes $3\frac{1}{2}$ of nitrous gas to ſaturate it, when in water. Perhaps it may be found that nitrous gas uſually contains a ſmall portion of nitrous oxide.

If water and gaſes be agitated in confinement, a mixture will take place of the gaſes in and out of the water, &c.

If water impregnated with any one gas (as hydrogenous) be agitated with another gas *equally* abſorbable (as azotic) there will *apparently* be no abſorption of the latter gas; just as much gas being found after agitation as was introduced to the water; but upon examination the reſiduary gas will be found a *mixture* of the two, and the parts of each, in the water, will be exactly proportional to thoſe out of the water.

6. If water impregnated with any one gas be agitated with another gas leſs or more abſorbable; there will *apparently* be an increaſe or diminution of the latter; but upon examination the reſiduary gas will be found a *mixture* of the two, and the proportions agreeable to article 4.

Temperature does not affect ſuch incloſed fluids.

7. If a quantity of water in a phial having a ground ſtopper very accurately adapted, be agitated with any gas, or mixture of gaſes, till the due ſhare has entered the water; then, if the ſtopper be ſecured, the phial may be expoſed to any variation of *temperature*, without diſturbing the equilibrium: That is, the quantity of gas in the water will remain the ſame whether it be expoſed to heat or cold, if the ſtopper be air-tight.

N. B. The phial ought not to be near full of water, and the temperature ſhould be between 32° and 212°.

Gaſes which are diſpoſed to combine.

8. If water be impregnated with one gas (as oxygenous), and another gas, having an affinity for the former (as nitrous), be agitated along with it; the abſorption of the latter gas will be greater, by the quantity neceſſary to ſaturate the former, than it would have been if the water had been free from gas.*

The abſorption by other liquids is the ſame as by water.

9. Moſt liquids free from viſcidity, ſuch as acids, alcohol, liquid ſulphurets, and ſaline ſolutions in water, abſorb the ſame quantity of gaſes as pure water; except they have an affinity for the gas, ſuch as ſulphurets for oxygen, &c.

The preceding articles contain the principal facts neceſſary to eſtabliſh the theory of abſorption: Thoſe that follow are of a ſubordinate nature, and partly deducible as corollaries to them.

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* One part of oxygenous gas requires 3.4 of nitrous gas to ſaturate it in water. It is agreeable to this that the rapid mixture of oxygenous and nitrous gas over a broad ſurface of water, occaſions a greater diminution than otherwiſe. In fact, the *nitrous* acid is formed this way; whereas, when water is not preſent, the *nitric* acid is formed, which requires juſt half the quantity of nitrous gas, as I have lately aſcertained.

Natural waters or rain contain the due ſhare of atmoſ. air; but corrupt water has leſs or no oxygen.

10. Pure diſtilled water, rain and ſpring water uſually contain nearly their due ſhare of atmoſpheric air: if not, they quickly acquire that ſhare by agitation in it, and loſe any other gas they may be impregnated with. It is remarkable, however, that water by ſtagnation, in certain circumſtances, loſes part or all of its oxygen, notwithſtanding its conſtant expoſition to the atmoſphere. This I have uniformly found to be the caſe in my large wooden pneumatic trough, containing about eight gallons, or 1$\frac{1}{3}$ cubic foot of water. Whenever this is repleniſhed with tolerably pure rain water, it contains its ſhare of atmoſpheric air; but in proceſs of time it becomes deficient of oxygen: In three months the whole ſurface has been covered with a pellicle, and no oxygenous gas whatever was found in the water. It was grown offenſive, but not extremely ſo; it had not been contaminated with any material portion of metallic or ſulphureous mixtures, or any other article to which the effect could be aſcribed.* The quantity of azotic gas is not materially diminiſhed by ſtagnation, if at all.—Theſe circumſtances, not being duly noticed, have been the ſource of great diverſity in the reſults of different philoſophers upon the quantity and quality of atmoſpheric air in water. By article 4, it appears that atmoſpheric air expelled from water ought to have 38 per cent. oxygen; whereas by this article air may be expelled from water that ſhall contain from 38 to 0 per cent. of oxygen. The diſappearance of oxygenous gas in water, I preſume, muſt be owing to ſome impurities in the water which combine with the oxygen. Pure rain water that had ſtood more than a year in an earthenware bottle had loſt none of its oxygen.

Why water by agitation abſorbs moſt oxygen from air.

11. If water free from air be agitated with a ſmall portion of atmoſpheric air (as $\frac{1}{15}$ of its bulk) the reſiduum of ſuch air will have proportionally leſs oxygen that the original: If we take $\frac{1}{15}$, as above, then the reſiduum will have only 17 per cent. oxygen; agreeably to the principle eſtabliſhed in article 4. This circumſtance accounts for the obſervations made by Dr. Prieſtley, and Mr. William Henry, that water abſorbs oxygen in preference to azote.

Diſappearance of gas by agitation under a jar.

12. If a tall glaſs veſſel, containing a ſmall portion of gas be inverted into a deep trough of water, and the gas thus confined by the glaſs and the water be briſkly agitated, it will gradually diſappear.

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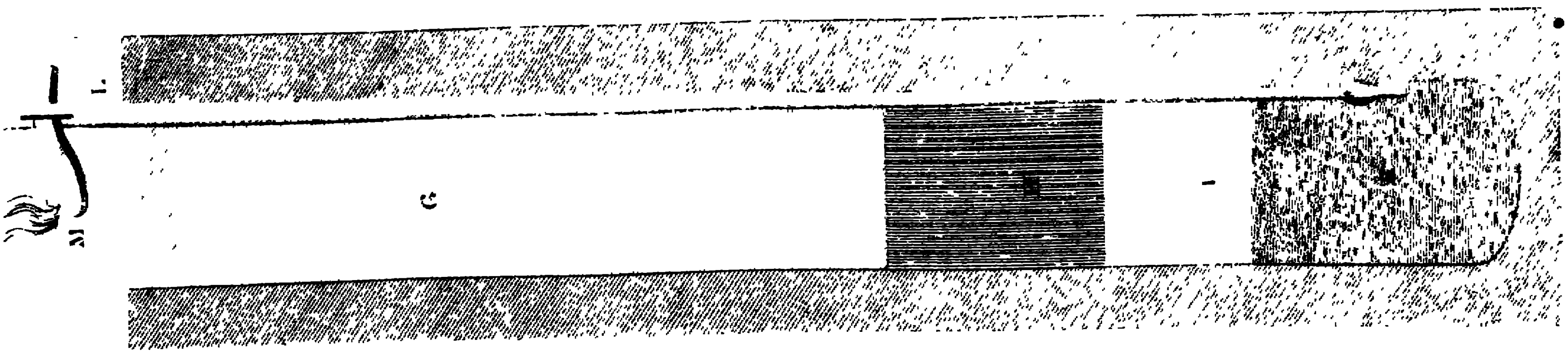
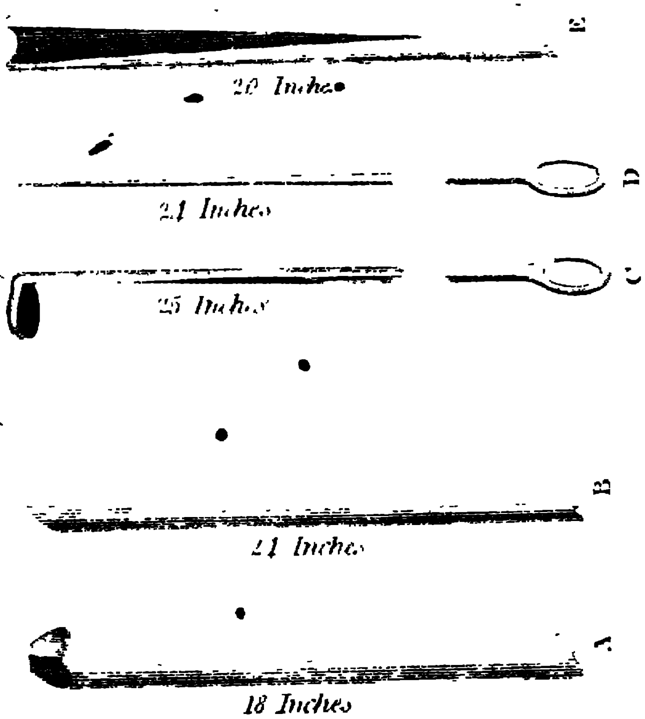
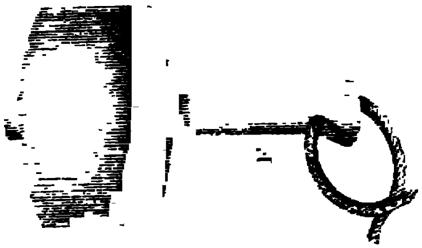
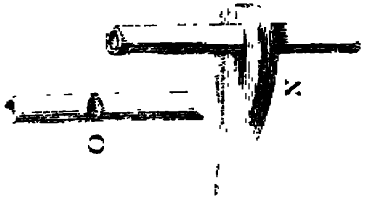
* It was drawn from a leaden ciſtern.

Method of blasting Rocks.

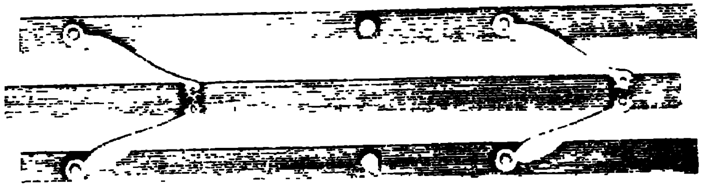








[illegible] by Mr Lowell



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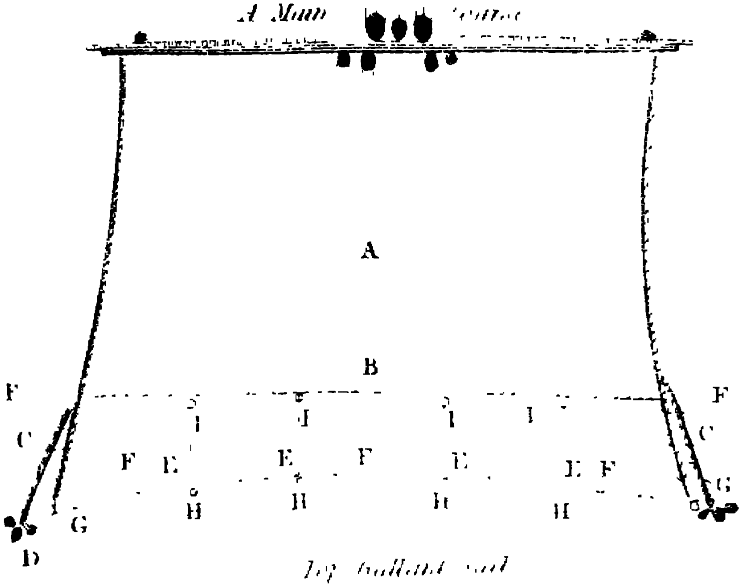
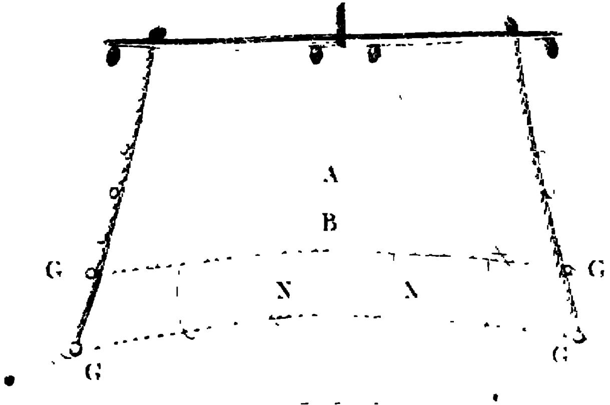
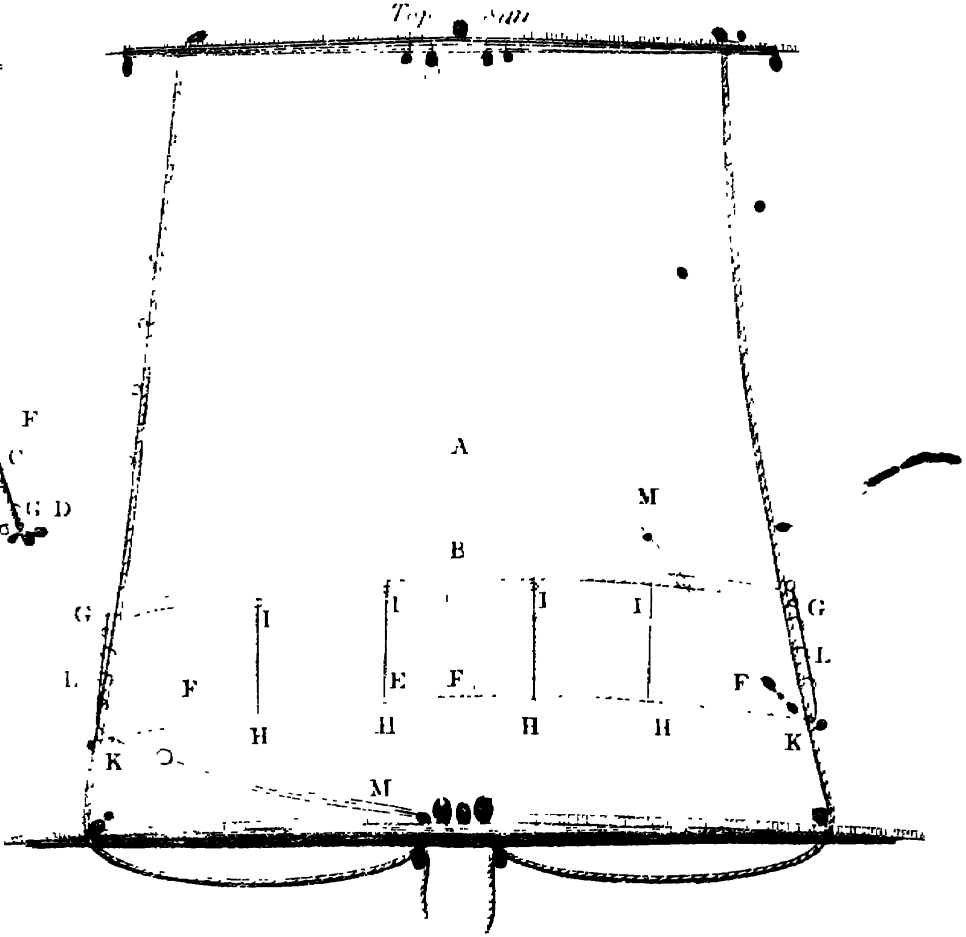
A Main Course



Top gallant sail



Top Sail



It is a wonder that Dr. Prieſtley, who ſeems to have been the firſt to notice this fact, ſhould have made any difficulty of it;—the loſs of gas has evidently a mechanical cauſe; the agitation divides the air into an infinite number of minute bubbles, which may be ſeen pervading the whole water; theſe are ſucceſſively driven out from under the margin of the glaſs into the trough, and ſo eſcape.

13. If old ſtagnant water be in the trough, in the laſt experiment, and atmoſpheric air be the ſubject, the oxygenous gas will very ſoon be almoſt wholly extracted, and leave a reſiduum of azotic gas; but if the water be fully impregnated with atmoſpheric air at the beginning, the reſiduary gas examined at any time will be pure atmoſpheric air.

Old ſtagnant water.

14. If any gas not containing either azotic or oxygenous gas, be agitated over water containing atmoſpheric air, the reſiduum will be found to contain both azotic and oxygenous gas.

Agitation of gas over common water gives out oxygen and azote by mixture.

15. Let a quantity of water contain equal portions of any two or more unequally abſorbable gaſes: For inſtance, azotic gas, oxygenous gas, and carbonic acid gas; then, let the water be boiled or ſubjected to the air-pump, and it will be found that unequal portions of the gaſes will be expelled. The azotic will be the greateſt part, the oxygenous next, and the carbonic acid will be the leaſt. For, the previous impregnation being ſuch as is due to atmoſpheres of the following relative forces nearly:

The eſcape of any gas from water by removing the preſſure will be greater the leſs abſorbable.

Azotic	- - -	21 inch. of mercury.
Oxygenous	- -	9
Carbonic acid	-	$\frac{1}{3}$

conſequently, when thoſe forces are removed, the reſiliency of the azotic gas will be the greateſt, and that of the carbonic acid the leaſt; the laſt will even be ſo ſmall as not to overcome the coheſion of the water without violent agitation.

Remarks on the Authority of the preceding Facts.

In order to give the chain of facts as diſtinct as poſſible, I have not hitherto mentioned by whom or in what manner they were aſcertained.

Remarks on the laws of abſorption of gaſes by denſe fluids, &c.

The fact mentioned in the firſt article has been long known; a doubt, however, remained reſpecting the quantity of air ſtill

Remarks on the laws of abſorption of gaſes by denſe fluids, &c.

left in water after ebullition and the operation of the air-pump. The ſubſequent articles will, I apprehend, have placed this in a clearer point of view.

In determining the quantity of gaſes abſorbed, I had the reſult of Mr. William Henry's experience on the ſubject before me, an account of which has been publiſhed in the Philoſophical Tranſactions for 1803. By the reciprocal communications ſince, we have been enabled to bring the reſults of our experiments to a near agreement; as the quantities he has given in his appendix to that paper nearly accord with thoſe I have ſtated in the ſecond article. In my experiments with the leſs abſorbable gaſes, or thoſe of the 2d, 3d, and 4th claſſes, I uſed a phial holding 2700 grains of water, having a very accurately ground ſtopper; in thoſe with the more abſorbable of the firſt claſs, I uſed an eudiometer tube, properly graduated, and of aperture ſo as to be covered with the end of a finger. This was filled with the gas and a ſmall portion expelled by introducing a ſolid body under water; the quantity being noticed by the quantity of water that entered on withdrawing the ſolid body, the finger was applied to the end and the water within agitated; then removing the finger for a moment under water, an additional quantity of water entered, and the agitation was repeated till no more water would enter, when the quantity and quality of the reſiduary gas was examined. In fact, water could never be made to take its bulk of any gas by this procedure; but if it took $\frac{9}{10}$, or any other part, and the reſiduary gas was $\frac{9}{10}$ pure, then it was inferred that water would take its bulk of that gas. The principle was the ſame in uſing the phial; only a ſmall quantity of the gas was admitted, and the agitation was longer.

There are two very important facts contained in the ſecond article. The firſt is, that the quantity of gas abſorbed is as the denſity or preſſure. This was diſcovered by Mr. William Henry, before either he or I had formed any theory on the ſubject.

The other is, that the denſity of the gas in the water has a ſpecial relation to that out of the water, the diſtance of the particles within being always ſome multiple of that without: Thus, in the caſe of carbonic acid, &c. the diſtance within and without is the ſame, or the gas within the water is of the ſame denſity as without; in olefiant gas the diſtance of the particles in the water is twice that without; in oxygenous gas, &c. the diſtance is juſt three times as great within as without; and in azotic, &c. it is four times. This fact was the reſult of my own enquiry. The former of theſe, I think, decides the effect to be mechanical; and the latter ſeems to point to the principle on which the equilibrium is adjuſted.

Remarks on the laws of abſorption of gaſes by denſe fluids, &c.

The facts noticed in the 4th, 5th and 6th articles, were inveſtigated *à priori* from the mechanical hypotheſis, and the notion of the diſtinct agency of elaſtic fluids when mixed together. The reſults were found entirely to agree with both, or as nearly as could be expected from experiments of ſuch nature.

The facts mentioned in the 7th article are of great importance in a theoretic view; for, if the quantity of gas abſorbed depend upon mechanical principles, it cannot be affected by temperature in confined air, as the mechanical effect of the external and internal air are alike increaſed by heat, and the denſity not at all affected in theſe circumſtances. I have tried the experiments in a conſiderable variety of temperature without perceiving any deviation from the principle. It deſerves further attention.

If water be, as pointed out by this eſſay, a mere receptacle of gaſes, it cannot affect their affinities: hence what is obſerved in the 8th article is too obvious to need explanation.—And if we find the abſorption of gaſes to ariſe not from a chemical but a mechanical cauſe, it may be expected that all liquids having an equal fluidity with water, will abſorb like portions of gas. In ſeveral liquids I have tried, no perceptible difference has been found; but this deſerves further inveſtigation.

After what has been obſerved, it ſeems unneceſſary to add any explanation of the 10th and following articles.

Theory of the Abſorption of Gaſes by Water, &c.

From the facts developed in the preceding articles, the following theory of the abſorption of gaſes by water ſeems deducible.

1. All gaſes that enter into water and other liquids by means of preſſure, and are wholly diſengaged again by the removal of that preſſure, are *mechanically* mixed with the liquid, and not *chemically* combined with it.

2. Gaſes

Remarks on the laws of abſorption of gaſes by denſe fluids, &c.

2. Gaſes ſo mixed with water, &c. retain their elaſticity or repulſive power amongſt their own particles, juſt the ſame in the water as out of it, the intervening water having no other influence in this reſpect than a mere vacuum.

3. Each gas is retained in water by the preſſure of gas of its own kind incumbent on its ſurface abſtractedly conſidered, no other gas with which it may be mixed having any permanent influence in this reſpect.

4. When water has abſorbed its bulk of carbonic acid gas, &c. the gas does not preſs on the water at all, but preſſes on the containing veſſel juſt as if no water were in. When water has abſorbed its proper quantity of oxygenous gas, &c. that is, $\frac{1}{27}$ of its bulk, the exterior gas preſſes on the ſurface of the water with $\frac{26}{27}$ of its force, and on the internal gas with $\frac{1}{27}$ of its force, which force preſſes upon the containing veſſel, and not on the water. With azotic and hydrogenous gas the proportions are $\frac{63}{64}$ and $\frac{1}{64}$ reſpectively. When water contains no gas, its ſurface muſt ſupport the whole preſſure of any gas admitted to it, till the gas has, in part, forced its way into the water.

5. A particle of gas preſſing on the ſurface of water is analogous to a ſingle ſhot preſſing upon the ſummit of a ſquare pile of them. As the ſhot diſtributes its preſſure equally amongſt all the individuals forming the loweſt ſtratum of the pile, ſo the particle of gas diſtributes its preſſure equally amongſt every ſucceſſive horizontal ſtratum of particles of water downwards till it reaches the ſphere of influence of another particle of gas. For inſtance; let any gas preſs with a given force on the ſurface of water, and let the diſtance of the particles of gas from each other be to thoſe of water as 10 to 1; then each particle of gas muſt divide its force equally amongſt 100 particles of water, as follows:—It exerts its immediate force upon 4 particles of water; thoſe 4 preſs upon 9, the 9 upon 16, and ſo on according to the order of ſquare numbers, till 100 particles of water have the force diſtributed amongſt them; and in the ſame ſtratum each ſquare of 100, having its incumbent particle of gas, the water below this ſtratum is uniformly preſſed by the gas, and conſequently has not its equilibrium diſturbed by that preſſure.

6. When water has abſorbed $\frac{1}{27}$ of its bulk of any gas, the ſtratum of gas on the ſurface of the water preſſes with $\frac{26}{27}$ of

Remarks on the laws of abſorption of gaſes by denſe fluids, &c.

its force on the water, in the manner pointed out in the laſt article, and with $\frac{1}{27}$ of its force on the uppermoſt ſtratum of gas in the water: The diſtançe of the two ſtrata of gas muſt be nearly 27 times the diſtance of the particles in the incumbent atmoſphere, and 9 times the diſtançe of the particles in the water. This comparatively great diſtance of the inner and outer atmoſphere ariſes from the great repulſive power of the latter, on account of its ſuperior denſity, or its preſenting 9 particles of ſurface to the other 1. When $\frac{1}{64}$ is abſorbed, the diſtance of the atmoſpheres becomes 64 times the diſtance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal ſtrata of gas in and out of water, will ſufficiently illuſtrate theſe poſitions.

7. An equilibrium between the outer and inner atmoſpheres can be eſtabliſhed in no other circumſtance than that of the diſtance of the particles of one atmoſphere being the ſame or ſome multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this caſe the diſtance of the inner and outer atmoſpheres is ſuch as to make the perpendicular force of each particle of the former on thoſe particles of the latter that are immediately ſubject to its influence, phyſically ſpeaking, equal; and the ſame may be obſerved of the ſmall lateral force.

8. The greateſt difficulty attending the mechanical hypotheſis, ariſes from different gaſes obſerving different laws.—Why does water not admit its bulk of every kind of gas alike? This queſtion I have duly conſidered, and though I am not yet able to ſatisfy myſelf completely, I am nearly perſuaded that the circumſtance depends upon the weight and number of the ultimate particles of the ſeveral gaſes: thoſe whoſe particles are lighteſt and ſingle being leaſt abſorbable, and the others more, according as they increaſe in weight and complexity.* An enquiry into the relative weights of the ultimate particles of bodies, is a ſubject, as far as I know, entirely new: I have lately been proſecuting this enquiry with remarkable ſucceſs. The principle cannot be entered upon in this paper; but I ſhall juſt ſubjoin the reſults, as far as they appear to be aſcertained by my experiments.

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* Subſequent experience renders this conjecture leſs probable.

Weights of the particles of bodies.

Table of the relative weights of the ultimate particles of gaſeous and other bodies.

Hydrogen	1
Azote	4.2
Carbon	4.3
Ammonia	5.2
Oxygen	5.5
Water	6.5
Phoſphorus	7.2
Phoſphuretted hydrogen	8.2
Nitrous gas	9.3
Ether	9.6
Gaſeous oxide of carbon	9.8
Nitrous oxide	13.7
Sulphur	14.4
Nitric acid	15.2
Sulphuretted hydrogen	15.4
Carbonic acid	15.3
Alcohol	15.1
Sulphureous acid	19.9
Sulphuric acid	25.4
Carburetted hydrogen from ſtagnated water	6.3
Olefiant gas	5.3

VI.

On the ſuppoſed faſcinating Power of the Rattle-ſnake. With a remarkable Indian Tradition upon which it is probable the early European Settlers founded their popular Tales. From the Philadelphia Medical and Phyſical Journal, by BENJAMIN SMITH BARTON, *M. D.*

Faſcinating power of the rattle-ſnake deſcribed by Fabricius.

ALMOST all amphibious animals (ſays Profeſſor Fabricius,) the tortoiſe excepted, live by preying upon other animals. But being deſtitute of ſtrength and ſwiftneſs, nature has given, at leaſt to ſome of them (according to the teſtimony of many and creditable writers,) the peculiar faculty of forcing other animals to throw themſelves into their open jaws. Kalm, the Swede, and the American Smith Barton, aſſert of the

Americans

American ſerpents, that if they fix their fiery, glaring eyes upon any animal, ſuch as a ſquirrel, or a bird, within a certain diſtance, they entirely loſe the power of eſcaping, but throw themſelves, ſlowly, irreſiſtibly, into the extended jaws of the ſnake. And if any thing diſturbs the ſnake, ſo that it withdraws its eyes but for one moment, they eſcape with the utmoſt precipitation.

We obſerve (continues this learned naturaliſt) ſomething ſimilar to this in our common, tardy, thick, and fat toads, which frequently ſit under little ſtones and buſhes, having their mouths wide open, into which flies, bees, and other inſects, are drawn in the ſame manner. All the theories that have hitherto been offered to explain theſe appearances appear to me both unnatural and improbable. Indeed, I cannot but doubt the reality of the fact itſelf, until we ſhall receive further obſervations and diſcoveries relative to it.

J. C. Fabricii, &c.
Reſultate Natur-Hiſtoriſcher Vorleſungen,
p. 267, 268. *Kiel:* 1804.

Annotation by Dr. Barton.

It will be evident to any one, who has peruſed, with attention, my two publications * on the ſuppoſed faſcinating faculty of the rattle-ſnake, and other American ſerpents, that Mr. Fabricius has by no means fully comprehended my peculiar theory. I have not adopted the hypotheſis of the very reſpectable Kalm, with whoſe name mine is mentioned by the Daniſh Profeſſor. On the contrary, I have endeavoured to ſhow, and I flatter myſelf that I have very ſatisfactorily ſhown, that there is no ſolid foundation for the vulgar, and very generally-received opinion, that ſerpents are endued with the faculty of faſcinating, or charming, other animals.

B. S. B.

Narrative.

The following very curious tradition of ſome of our Indians, relative to ſerpents, is worthy of publication in this place. A *part* of the tradition has already been publiſhed in my

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* A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-ſnake, and other American Serpents. Philadelphia: 1796.—Supplement to a Memoir, &c. Philadelphia, 1800.—Or ſee Philoſ. Journal, Vols. VII. and VIII.

Supplement to a Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-ſnake, and other American Serpents.

The rattle-ſnake catches its prey by craft and addreſs.

‘ Having queſtioned Indians, a number of times, with reſpect to ſnakes having the power of charming, and always being anſwered in the negative, I was at length deſired (ſays my friend, Mr. John Heckewelder) to give the reaſon the white people had for believing ſuch a thing, which not being ſatisfactory, Pemaholend * declared: “ The rattle-ſnake obtains its food merely by ſlyneſs, and a perſevering patience. It knoweth as well where to watch for its prey as a cat does, and ſucceeds as well. It has, and retains its hunting grounds. In ſpring, when the warm weather ſets in, and the woods ſeem alive with the ſmaller animals, it leaves its den. It will croſs a river, and go a mile and further from its den, to the place it intends to ſpend the ſummer; and in fall, when all the young animals bred this ſeaſon are become ſtrong and active, ſo that they are no more ſo eaſily overtaken or caught, it directs its courſe back again, to its den, the ſame as a hunter does to his camp.

Indian tradition.

“ The white-people,” continued Pemaholend, “ probably have taken the idea of this ſnake having the power of charming from a tradition of ours (the Indians) which our forefathers have handed down to us, from many hundred years back, and long before ever the white people came into this country. Then (they tell us) there *was* ſuch a ſnake, and a rattle-ſnake too, but then there was only *this one* ſnake which had this power, and he was afterwards deſtroyed; and ſince that time it hath never been ſaid that any other of the kind had made its appearance.”

American native tradition about a rattle-ſnake.

‘ At my requeſt, Pemaholend related the tradition, and in the following words. “ Our forefathers have told us, that at a ſmall lake, or large pond, not a great diſtance from where, as is believed, now the great city *Quequenàku* (Philadelphia) is built, there dwelt a rattle-ſnake, whoſe length and thickneſs exceeded that of the thickeſt and longeſt tree in the woods. This ſnake was very deſtructive, not only in deſtroying ſo much game, but in devouring ſo many Indians: for when he was hungry, he only looked round, and whatever he

ſaw,

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* An aged and much reſpected Delaware-Indian.

American native tradition about a rattle-ſnake.

ſaw, whether Indian, deer, turkey, or even geeſe flying, he only held his head that way, opening his mouth wide, and drawing breath in the manner we do, and nothing could prevent ſuch living creature entering his jaws. It is even ſaid, that a whole flock of geeſe, flying at a great diſtance, have been drawn into his mouth, at one time; * and it was well known among the Indians, that of all the hunters or travellers, who paſſed that way, very few eſcaped him.

"The Indians well knew when he was hungry, for then he grew angry, and blew with his mouth, which ſounded like thunder: for his breath was ſo powerful, that all the trees, however large, would bend, and even ſometimes break down before him. There being no proſpect of ever killing him with arrows, on account of the barrenneſs of the land far round the lake, into which he would always retire, after ſatisfying his hunger, a great council of the nation was called together, and the queſtion put, *Where are the Mannittoes of the nation? Are they no more? Shall the whole of the nation be deſtroyed by a Mannitto-Snake?* At length, two young men, endowed with *Mannittoie powers*, offered their ſervices, and declared, that unleſs the Mannittoie power of the ſnake exceeded theirs, they ſhould ſucceed; but they would, at all events, make an attempt. They then bid farewell to the aſſembly and their friends, dived into the river, from whence they proceeded under the water to a place oppoſite the Mennúppeek (lake, or large pond) where this ſnake dwelt. They made an opening under ground, from the river to the centre of the pond, by which the pond was drained, and became perfectly dry. After returning again, the ſame way they had come, they found the ſnake in great uneaſineſs, and on dry ground. Taking then the advantage of the dry weather, and the graſs far around the ſnake being dry, they ſet fire to the graſs, at a diſtance, and around the ſnake, by which means he was burnt

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* It is curious, at leaſt, to compare this part of the Indian tradition with what Metrodorus, as cited by Pliny, relates of certain Aſiatic ſerpents. Theſe, he ſays, by means of their breath, attracted birds, however high they were, or however quick their flight. "Metrodorus, circa Rhyndacum amnem in Ponto, ut ſupervolantes quamvis alte perniciterque, alites hauſtu raptas abſorbeant." Plin. Hiſt. Nat. lib. viii. cap. 14.

American native tradition about a rattle-ſnake.

to death.* Thus (continued Pemaholend) was the monſter killed by two mannitto men of the nation: for, you muſt know, in thoſe days, we had ſuch men among us, who could live as well in the water as on land."

'Converſing one day with a Monſy (advanced in years) on ancient times, on the migration of the Indians, &c. he, in order to convince me (ſays Mr. Heckewelder) what the Indians once were, mentioned the killing of the big ſnake, the hiſtory of which, according to his relation, differing only in the following points:

"*a.* He did not think it had been a rattle-ſnake, but underſtood the old men, from whom he had heard it ſo often related (when he was young), that it was a ſnake of a peculiar kind, and had feet; and that never ſince had a ſnake of this kind appeared:

"*b.* That he was not ſure as to the place where this ſnake kept; believed it had been higher up the country, and kept in a wide and deep place of the river, and in the country of the Munſees (or Minſy) and was killed by a Mannitto Munſee:

"*c.* That after the nation had met in council, and the above queſtions put, a Munſee man of no character, nor ſeemingly of any conſequence to the nation, ſaid and declared, that he had *Mannittoie Powers;* could and would deſtroy the monſter, preſcribing the ceremonies the aſſembly were to obſerve during the expedition. That he then made a very ſtrong arrow, or ſpear, ſharp at both ends; and being equipped, took leave of the aſſembly—plunged into the river, and dived under water, until he arrived within a ſmall diſtance of the place where the ſnake lay, or floated, baſking in the ſun. Here he aſcended to the ſurface, and calling out to the ſnake to receive him, he opened his mouth wide, and drew him in, when, however, in an inſtant, the ſnake was ſtabbed by him through both his ſides, with the ſpear, which wounded him ſo deadly, that he gave a whirl, and being under great pain, diſcharged his excrements, and with the ſame this hero, who then ſwam again to ſhore, announcing his victory, and congratulating the aſſembly on the deliverance of the nation.

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* Even this part of the Indian tradition ſeems to be borrowed from the old world. See a curious relation of the capture of an enormous ſerpent in *The Life of Sethos, as taken from private memoirs of the Egyptians.* Vol. i. p. 125—147. London, 1737.

"Thus (continued the old Munſee) were the Indians of thoſe days *Mannittoes.* Nothing could reſiſt them. They knew nothing of drowning. Our firſt Parents have ſprung from the bottom of a lake."

VII.

A Deſcription of a Property of Caoutchouc, or Indian Rubber; With ſome Reflections on the Cauſe of the Elaſticity of this Subſtance. In a Letter to Dr. HOLME.*

SIR, *Middleſhaw, near Kendal, Nov.* 26, 1802.

THE ſubſtance called Caoutchouc, or Indian Rubber, poſſeſſes a ſingular property; which, I believe, has never been taken notice of in print, at leaſt by any Engliſh writer; the preſent letter contains my experiments and reflections on the ſubject; and ſhould they appear to deſerve the attention of your philoſophical friends, I am certain you will take the trouble of communicating the paper to the Literary and Philoſophical Society of Mancheſter.

Caoutchouc more pliant by heat,

The property I am about to deſcribe depends on the temperature of the Caoutchouc, which is uſed in the experiment; for heat increaſes the pliancy of the ſubſtance, and cold, on the contrary, renders it more rigid: ſo that when a ſlip of this reſin has been ſufficiently warmed, it may be extended to more than twice its natural length, by a moderate force applied to its extremities, after which it will recover its original dimenſions in a moment, provided one of the ends of it be let go as ſoon as it has been ſtretched. This diſpoſition of the ſubſtance may be produced by a degree of temperature leſs than the heat of the blood; it is therefore neceſſary to prepare a ſlip of it, by ſteeping it for a few minutes in warm water, or by holding it ſomewhat longer in the fiſt; either of theſe precautions makes the reſin pliant, and fits it for the experiment; which is performed in the following manner.

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* Mancheſter Mem. N. S. Vol. I.

and leſs denſe.

I made a piece of Caoutchouc a little heavier than an equal bulk of water, the temperature of which was 45 degrees: the veſſel containing the reſin and water was then placed on the fire; and when the contents of it were heated to 130 degrees, the Caoutchouc floated on the ſurface.

It becomes cold by ſudden drawing out, and hot by contraction.

Exp. 1. Hold one end of the ſlip, thus prepared, between the thumb and fore-finger of each hand; bring the middle of the piece into ſlight contact with the edges of the lips;* taking care to keep it ſtraight at the time, but not to ſtretch it much beyond its natural length: after taking theſe preparatory ſteps, extend the ſlip ſuddenly; and you will immediately perceive a ſenſation of warmth in that part of the mouth which touches it, ariſing from an augmentation of temperature in the Caoutchouc: for this reſin evidently grows warmer the further it is extended; and the edges of the lips poſſeſs a high degree of ſenſibility, which enables them to diſcover theſe changes with greater facility than other parts of the body. The increaſe of temperature, which is perceived upon extending a piece of Caoutchouc, may be deſtroyed in an inſtant, by permitting the ſlip to contract again; which it will do quickly by virtue of its own ſpring, as oft as the ſtretching forth ceaſes to act as ſoon as it has been fully exerted. Perhaps it will be ſaid, that the preceding experiment is conducted in a negligent manner; that a perſon, who wiſhes for accuracy, will not truſt his own ſenſe of feeling in inquiries of this deſcription, but will contrive to employ a thermometer in the buſineſs. Should the objection be ſtarted, the anſwer to it is obvious; for the experiment in its preſent ſtate demonſtrates the reality of a ſingular fact; by convincing that ſenſe, which is the only direct judge in the caſe, that the temperature of a piece of Caoutchouc may be changed, by compelling it to change its dimenſions. The uſe of a thermometer determines the relative magnitudes of theſe variations, by referring the queſtion of temperature to the eye; experiments of this ſort are therefore of a mathematical nature, and afford a kind of knowledge with which we have nothing to do at preſent; for we are not inquiring after proportions, but endeavouring to eſtabliſh the certainty of a fact, which may aſſiſt in diſcovering the reaſon of the uncommon elaſticity obſervable in Caoutchouc. My eſſay or letter appears to be running into a long digreſſion; the ſubject muſt therefore be reſumed, and it will not be improper to premiſe the following ſimple experiment, in the preſent ſtate of the inquiry; becauſe it ſeems capable of affording no inconſiderable degree of inſight into the plan which nature purſues in producing the phenomenon in queſtion.

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* This effect was firſt noticed in 1784, at Mr. Kirwan's meetings in Newman ſtreet, and Dr. Crawford aſcribed it to change of capacity ſimilar to what he ſuppoſed to take place in a nail by hammering.—N.

Caoutchouc when ſtretched expands by heat and contracts by cold.

Exp. 2. If one end of a ſlip of Caoutchouc be faſtened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical poſition; the thong will be found to become ſhorter with heat and longer with cold. The proceſſes of heating, cooling, and meaſuring bodies are ſo well known, that I need not enter into the minuter parts of the experiment; it will be proper, however, to add, that an increaſe of temperature diminiſhes the ſpecific gravity of the Indian Rubber, and a loſs of heat occaſions a contrary effect in it; as I have proved experimentally. The knowledge of the latter fact leads me to conclude, apparently on reaſonable grounds, that the pores or interſtices of Caoutchouc are enlarged by heat, and diminiſhed by cold; conſequently when a ſlip of this ſubſtance which remains extended by a weight, or the application of force, happens to contract from an acceſſion of temperature, the capacity of its pores, taken ſeparately or collectively, is augmented by the change that takes place in the figure of the thong. Now if the exiſtence of caloric be admitted, it will follow from the preceding arguments, that the phenomenon under conſideration is occaſioned by the alternate abſorption and emiſſion of the calorific fluid, in the ſame manner that ropes, the blades of Fuci, as well as many more bodies, are obliged to contract and extend themſelves, by the alternate abſorption and emiſſion of water.—You will perceive by the tenour of the foregoing obſervations, that my theory of this caſe of elaſticity is perfectly mechanical; in fact, the explanation of it depends upon the mutual attraction of Caloric and Caoutchouc; the former of which penetrates the latter, and pervades every part of it with the greateſt eaſe and expedition; by which the reſin is compelled to accommodate its pores to that portion of the Calorific fluid which is due to its whole maſs,

Theory. That this ſubſtance is affected by caloric, as ropes are by water.

that its capacity may be mechanically altered and the caloric extruded, &c.

at any particular degree of temperature. In order to apply the laſt remark to the phenomenon under conſideration I may obſerve, that if a force be exerted on a piece of Caoutchouc to alter the dimenſions of its pores, the mutual attraction mentioned above will reſiſt the effort. But the eaſe with which this ſubſtance may be made to change its figure, and the retractile power which it poſſeſſes on theſe occaſions, ſhew that its conſtituent particles move freely amongſt themſelves: but where there is motion, there is void ſpace; conſequently Caoutchouc abounds with innumerable pores or interſtices, the magnitudes of which are variable, becauſe the ſpecific gravity of the reſin becomes leſs with heat, and greater with cold. Now if the dimenſions of the pores in a piece of Caoutchouc can be leſſened, without taking away part of the matter of heat, which it contains at the time; this new arrangement in the internal ſtructure of the ſlip will leſſen its capacity for the matter of heat, and conſequently augment its temperature. But the warmth of ſuch a ſlip is increaſed by ſtretching it, according to the firſt experiment; the pores of it are therefore diminiſhed: and the effort, which it exerts at the time, ariſes from the mutual attraction of the Caoutchouc and Caloric; which attraction cauſes an endeavour to enlarge the interſtices of the former for the reception of the latter; hence it happens that the thong contracts longitudinally, according to the ſecond experiment, and the redundant caloric is abſorbed in the courſe of this operation, which again reduces the temperature. The preceding explanation agrees very well with the phenomenon, as it is ſtated in the beginning of this letter; and the theory receives additional confirmation from the following facts.

Overſtretched Caoutchouc does not completely recover itſelf in the cold; but heat reſtores its elaſticity:

Exp. 3. If a thong of Caoutchouc be ſtretched in water warmer than itſelf, it retains its elaſticity unimpaired; on the contrary, if the experiment be made in water colder than itſelf, it loſes part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of ſpring, and the heat will immediately make it contract briſkly. The foregoing circumſtances may be conſidered as proving, that the elaſticity of Caoutchouc is not a conſtitutional quality of the ſubſtance, but a contingent effect, ariſing from the loſs of equilibrium between the portion of caloric, which the

whence the nature of its elaſticity is deduced, &c.

reſin happens to contain at any moment, and its capacity to receive that fluid at the ſame inſtant. The object of the preſent letter is to demonſtrate, that the faculty of this body to abſorb the calorific principle, may be leſſened, by forcibly diminiſhing the magnitudes of its pores; and this eſſential point of the theory may be confirmed by experiment: for the ſpecific gravity of a ſlip of Caoutchouc is increaſed, by keeping it extended, while it is weighed in water.

JOHN GOUGH.

VIII.

Obſervations on the training of Pugiliſts, Wreſtlers, Jockies, and others, who give themſelves up to Athletic Exerciſes; with ſome Queries for diſcovering the Principles thereof, and the Proceſs of training Running Horſes, &c. with a View of aſcertaining whether the ſame can furniſh any Hints ſerviceable to the Human Species. *

General conſiderations on organic diſeaſes.

PROFESSIONAL men are ready to acknowledge, that prevention is better than cure; and the beſt informed ingenuouſly admit, that organic diſeaſes, once confirmed, are beyond the reach of their art. As organic diſeaſes generally proceed from ſlow and gradual changes, they may certainly be prevented by temperance and labour; by activity of body, and contentment of mind. In regard to the common metaphyſical expreſſions, "of the exhauſting of the excitability; of the wearing of the parts; of the attrition of our fluids, in circulation, againſt the ſolids; of the abraſion of the ſolids by friction;

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* The ſubſequent queries and obſervations have been circulated by Sir John Sinclair, with a view to obtain information concerning the effects of diet and exerciſe on the human frame, from a claſs of practical experimentaliſts, whom the pride of ſcience has hitherto overlooked. The philoſophical manner in which this branch of diætatic medicine is here conſidered, appears to render it a fit object for inſertion in a Journal conducted on the plan of the preſent. In promoting the circulation of this paper, we have no doubt that we are coinciding with the plan of the author, by extending his means of information: Any communications tending to throw further light on the ſubject, will be acceptable. W. N.

tion; of the debility produced by the moſt natural powers ſupporting life, namely, the waſte of ſubſtance created by that exerciſe and labour, for which we ſeem peculiarly deſtined,"—all theſe expreſſions are extremely ſuſpicious. The ſpeculator is always to be ſuſpected, when, forſaking plain direct facts, he involves his want of meanings and conſcious ignorance, in learned words, or metaphor.

It is uſually ſuppoſed that bones, and other ſolid parts are permanent; but they are ſucceſſively replaced, like the fluids.

Theſe metaphorical expreſſions have originated in a perſuaſion, that the bones, cartilages, muſcles, and other ſolid parts, being once formed, *are permanent*, becauſe the identity of the individual is permanent; and that being once formed, and always retaining one ſhape, their actual component parts muſt continue the ſame. Nothing in philoſophy is farther from the truth. There are experiments to demonſtrate, that every part and particle of the firmeſt bones, is ſucceſſively abſorbed and depoſited again*. The ſolids of the body, whatever their form or texture, are inceſſantly renewed. The whole body is a perpetual ſecretion, and the bones and their ligaments, the muſcles and their tendons, all the finer and all the more flexible parts of the body, are as continually renewed, and as properly a ſecretion, as the ſaliva that flows from the mouth, or the moiſture that bedews the ſurface. The health of all the parts, and their ſoundneſs of ſtructure, depends on this perpetual abſorption, and perpetual renovation; and exerciſe, by promoting at once abſorption and ſecretion, promotes life, without hurrying it; renovates all the parts and organs, and preſerves them apt and fit for every office.

Nutrition is a general proceſs.

Nutrition belongs not to the ſtomach alone, which but prepares the food, and converts it into chyle, but to the veſſels by which it is circulated, and appropriated to the nutrition of parts, which of courſe is performed by every petty artery of the body.

Many general rules are raſh and dangerous.

In nothing ſhould we be more anxiouſly careful, than in laying down rules, which muſt affect the health of thouſands; and whenever we proceed on doctrines, unſupported by fact, wherever we divert mankind from thoſe amuſements and labours to which nature excites us, we ſhould proceed with particular caution. We read in books, that life and the body are but as a given quantity of living energy and living materials, to be expended and uſed with diſcretion and economy; and that the ſum of excitability, which is born with the child, is expended towards the cloſe of life. The doctrine of abraſion alſo intimates, that our ſolids are perpetually waſting, and that it is by the diminution of moiſture,—the aridity of ſolids, the ſcantineſs of fluids, and the ſlow induration of the ſolid parts; that the body becomes ſhrunk, emaciated, ſtiff, and motionleſs, before it ſinks into the grave. And, raſh as the doctrine ſeems, it has been boldly aſſerted, that "to live with as little food, and as little exerciſe as poſſible, is the ſureſt means to preſerve the body, and to live long." To live with as little food, and as little exerciſe as poſſible, would make a man little better than a mere graſshopper. A man living thus, would be a voluntary priſoner, wan, colourleſs, fleſhleſs, bloodleſs, having no ſpeculation in his eyes, no marrow in his bones; his complexion would declare him what he was. This ſyſtem practiſed, either in infancy, in the prime of manhood, or in the decline of life, would abridge it. Aſcetics are a proof, not of the length of life, which temperance inſures, but of the premature old age which abſtinence brings upon us. The ſqualid look, the hollow cheek, the matted hair, the emaciated body, only prove how much, by ſuch criminal ſelf-denial, the body ſuffers, with but little profit to the powers of the mind. Let us then take care that our philoſophy be not too ſevere; for men may run into real danger, if we take from them every fair indulgence, or divert them from following the dictates of nature. The faireſt livers, who have not abuſed, but have enjoyed their ſtrength and health, have in general enjoyed them longeſt.

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* This has been aſcertained by giving madder to growing animals, eſpecially pigs and fowls, among their food. It is found that the madder tinges the bones, layer after layer, with a red colour; and by the deepneſs of the tinge, demonſtrates the ſucceſſion in which the particles of the bone are abſorbed and depoſited. This is, I believe, the concluſion which phyſiologiſts have formed.

The doctrine of abraſion or wearing out has been abſurdly applied.

Natural habits of infancy, youth, &c. &c.

There are habits which ſeem to be natural to, and congenial with, the ſeveral periods of life. The child ſhould merely ſuck, ſleep, and vegetate. The boy ſhould ramble wild and unconſtrained, little oppreſſed with taſks or ſtudies, and nouriſhed with abundance of ſimple food. The youth ſhould be temperate, ſober, active. The old man quiet, ſedate, ſelf-indulgent; ſhould have long ſleep, delicate food, rich wines, and agreeable temperature; little labour, and a cheerful mind.

Nature aſſigns us vigour, ſpirit, enterpriſe, and foreſight in the early part of life, to treaſure up the needfull indulgences for age. Parents are careful of our firſt infancy; we ourſelves ought to provide for our latter childhood.

Conſiderations reſpecting the functions of the ſkin;—

The moſt intelligent profeſſional men have an opinion concerning the functions of the ſkin, conſonant with that of the vulgar; and more refined, only from their aſſigning a general cauſe for thoſe effects, of which all of us are conſcious. The ſkin is not regarded merely as an organ of ſecretion, deſtined for draining off ſuperfluous moiſture, or ſaline particles, from the general maſs of fluids, but as a ſurface of more active circulation, which ſolicits the blood to the very extremities of the veſſels, and thus contributes to ſupport and complete the circulation of the blood, and to nouriſh the parts within. The ſkin is regarded as connected, in a peculiar manner, with all the parts of the cellular ſubſtance, interpoſed betwixt the muſcles, and involving the blood veſſels. The ſtate of the ſkin indicates the condition of that cellular ſubſtance, whoſe office it is to conduct the blood-veſſels to all parts, eſpecially to the muſcular fleſh, and to nouriſh the parts; and while the circulation of the ſkin is lively and active, that of the involved parts can never flag. The condition of the bowels, and of the ſkin, are the firſt and moſt natural points for the phyſician to attend to. It is by regulating theſe, that he regulates the pulſe; by ſtimulating or ſoothing them, that he raiſes or depreſſes the vital actions; and it is matter of common obſervation, that in animals, a good ſkin is the criterion of health, and the dryneſs of the ſkin, the forming of ſcabs or eruptions upon it, and the clapping of the hair, (as it is called by thoſe who have the care of ſtock), are the firſt and ſureſt ſigns of approaching diſeaſe.

—and the inteſtines.

The lungs and their office.

Next to the free circulation of the blood through all the body, terminating in the ſurface, that of the free tranſit of the blood through the lungs, is eſſential to health.

The oxydation or chemical change produced by air upon the blood, is eſſential to its vital properties. A free and powerful reſpiration is moſt eſſential to a freſh colour of the face, to lively ſpirits, and cheerful feelings, and to the healthy and vigorous actions of the body. "It is my breathing hour of the day," ſays Hamlet to Oſric. It is a princely thing to ſet apart hours for exerciſes; and there is little doubt, that if all thoſe, who linger away their hours in luxurious and indolent relaxations, were to aſſign a regular portion of their time to the hardy and manly exerciſes of walking, riding, fencing, &c. and would take *their breathing hour*, they would breathe long and well.

The art of training men to athletic exerciſes is wonderfully effective.

Theſe reflections naturally ariſe upon conſidering the almoſt incredible perfection, to which thoſe, whoſe profeſſion it is to train men to athletic exerciſes, have brought their reſpective arts. By certain proceſſes, they improve the breath, the ſtrength, and the courage of thoſe they take in hand, ſo as to enable them to run thirty, or walk a hundred miles, in a given ſpace of time; to excel in wreſtling; or to challenge a profeſſed boxer. Would it not then be a moſt important addition to the facts we already know concerning the means of improving ſtrength, and enſuring long life, if authentic information could be procured from thoſe diſtricts where athletic exerciſes prevail, what are eſteemed the beſt and ſureſt proceſſes for training men for foot-races, trials of ſtrength in wreſtling or boxing matches, or for raiſing the ſtrength and courage of game-cocks, or improving the wind, ſtrength, and ſpeed of running horſes to their higheſt pitch.*

Some account of the methods,

Thoſe who give themſelves out as ſkilful in this art, attend to the ſtate of the bowels, the ſkin, and the lungs. They uſe ſuch means as reduce the cellular or fatty ſubſtance, and invigorate the muſcular fibres. When they take a man in training for any feat of this kind, he is not oiled and ſuppled as the ancient athletics were; for as their common modes of life were hardy and active, they needed no other preparation: but he is ſweated, purged, and dieted, and then put upon trial. He is purged with very draſtic purges, to reduce his groſſneſs. He is made to walk out under a load of clothes; his walks are regularly increaſed, and a certain number of times a-week; he is laid between two feather-beds; ſweat promoted by drinks; his limbs taken from between the feather-beds, ſucceſſively, and rubbed very roughly. After enduring for many hours this ſtate of ſuffocation, he is comforted with a draught of ale or wine. The purges and ſweatings are repeated, according to the groſſneſs of his habit, and from time to time his trainer, (regarding him no otherwiſe than he would a running horſe, under the like diſcipline,) takes him out, and makes trial of his wind and ſtrength, and does not ceaſe till he has made him as lank as a greyhound, and almoſt as fleet. A man, even in the beſt of ordinary health, becomes giddy and breathleſs when he ſtrikes; and ſick and pale on receiving a few blows. He is thence unable to bear any unuſual exertion, and by inference prone to diſeaſe. If, by extenuating the fat, emptying in the cellular ſubſtance, hardening the muſcular fibres, and improving the breath, a man of the ordinary frame may be made to fight for one hour, with the utmoſt exertion of ſtrength and courage; the inquiry which I have already ſuggeſted muſt be of the higheſt uſe. For were this new train of facts regularly laid before profeſſional men, and were they enabled thus to judge of the influence which the methods of theſe practical philoſophers have on regulating the functions of breathing, perſpiration and digeſtion; it would be drawing into the province of ſcience, an art connected moſt particularly with the means of prolonging life, and hitherto known and practiſed only by a few inſulated individuals, of courſe imperfectly known, and of too limited uſe.

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* Though not immediately connected with the object of this paper, it may not be improper to ſuggeſt, that it would be of great importance, if medical gentlemen, whether in the army or navy, who have been on ſervice, were alſo to point out the various circumſtances which tended to ſupport, or to abate, the ſtrength and courage of the ſoldier or the ſailor.

and the great increaſe of force acquired by the human frame.

Theſe facts are probably of great value to the ſcience of prolonging life.

The art ſeems to be modern.

I queſtion whether the athletics of old uſed ſimilar means; whether they were equally ſucceſsful; whether there ever were, in any climate, age, or country, more hardy or powerful frames than thoſe of our Engliſh pugiliſts. In Cooke's voyage, we are told of the marked inferiority of the Engliſh ſailors, in wreſtling or boxing, to the naked ſun-burnt heroes of the South Sea Iſlands. But an Engliſh ſailor, though full of ſpirit and vigour, is as clumſy as a clown, and could not even row againſt an inhabitant of the Sandwich Iſlands. An Engliſh bricklayer, blackſmith, or drayman, however, who liked the ſport, and was practiſed in balancing and ſtriking, might have challenged the whole of the tawny nation.

Queries.

With a view of collecting ſuch important information, I am very anxious that the following queries ſhould be propoſed to theſe who profeſs the art of training pugiliſts, wreſtlers, and runners of foot-races, by ſuch intelligent men as have the opportunity of converſing with them.

Teſts of ſtrength &c.?

1. By what criterions or they judge of the muſcular ſtrength, or wind, or other alities of thoſe who ſeek to put themſelves under training. What is the earlieſt, and what is the lateſt age they would attempt to train?

Time required to train?

2. How they judge of the length of time that may be required for bringing a man into good plight, vigorous health, and free breathing; and what period of preparation is uſually required for running a match?

Purges, treatment, their object, &c.?

3. What purges they uſe; and in what ſucceſſion; and by what rules do they adminiſter them; and how do they judge of their effects? Is the purging only preparatory, or is it regularly continued? Is it meant, by this proceſs to reduce the plethoric ſtate of the ſyſtem, (on the idea that there is too great a quantity of blood,) or is it ſimply deſigned to put the bowels in the moſt favourable condition, for eaſy and good digeſtion? Is the reducing the actual ſize of the belly, neceſſary to more free and perfect breathing *?

Diet?

4. Is the diet rich or ſimple; of animal food, or of vegetable; in great quantity, or ſparing; is it increaſed gradually, or diminiſhed gradually? What meals have they in the day; and at what hours; one or more; frequent feeding, in ſmall and fixed portions, or full and ſubſtantial meals? What kinds of fleſh or meat is reckoned the beſt; whether beef, mutton, veal, pork, lamb, or fowl? Are any kinds of fiſh allowed? What quality of food is moſt conducive to ſtrength? What quantity is neceſſary for maintaining the ſyſtem in its moſt perfect ſtate of vigour? Do they feed much in the intermediate days of the purges? Is abſtinence required when they take their phyſic?

Liquors?

5. What kinds of liquors are reckoned beſt? Whether wine, ale, water, ſpirits, &c.? Whether given hot or cold; in what quantities; and when ought they to be given?

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* The effects of taking up a running horſe from idleneſs and ſoft paſture, to hard food and regular exerciſe, is attended with this peculiar effect, that while the animal becomes lank, ſleek, and gloſſy, while he gets fire in his eye, and a new vigour in his limbs, and wind and ſpeed, his belly, (ſwollen with coarſe indigeſtible food, eaten in great profuſion,) is drawn into half its ſize. May we not then preſume from this analogy, that the ſtate of the belly has a remarkable effect upon the wind.

Intention of the perſpirations, how excited, &c.?

6. Are the very violent perſpirations into which they throw their patients, deſigned to reduce the ſyſtem, to extenuate the fat, to leſſen that quantity of blood, the exceſs of which makes us giddy or ſhort breathed; or is it merely deſigned to produce a new condition of the ſkin, more favourable to health and muſcular vigour; to produce a ſharper appetite; a greater demand for food; and a quicker nouriſhment, or a greater nutrition from a more ſlender diet? Is the ſweat at firſt produced by exerciſe, and only continued by the perſon, when trained, being put between feather beds, and encouraged by drinks; or is it produced by force of ſweating drugs, or violent heats, or by continued friction? At what hours are the perſpirations brought on? How is the pupil treated when the ſweat is over? What becomes of the ſkin of a fat man, when, by the proceſs, he is reduced in ſize, and rendered lean? Does it hang looſe, or is it tight? Has it any effect upon the bones?

Exerciſe and treatment?

7. What hours of exerciſe do they require of their pupils during the day? At what hours do they ſend them out in the morning? How long do they continue abroad? Are they loaded with clothes after the body is reduced, and becomes limber, and thin and muſcular; or only while the ſweating proceſs continues? Are they fed before they go abroad, or when they return? What trials are made of their ſtrength? When is a man known to be up to his full ſtrength and breath in training? At what hours do they go to bed? What ſleep are they allowed? What indiſpoſitions are they ſubject to during training? Are there any circumſtances by which the proceſs may be interrupted; or any circumſtances, in conſequence of which, it muſt ſometimes be abandoned?

Subſequent effects of training?

8. What is the ſtate of the health, after they give up training? Are they ſubject to any complaints; and what are they? How long does the acquired exceſs of ſtrength continue?

What part of training moſt effectual? Whether it be permanent, temporary, curative

9. It is moſt intereſting to learn, on which part of this proceſs, the purging, the ſweating, the exerciſe, or the feeding, they moſt depend; and whether it procures a permanent increaſe of vigour, eaſily maintained by ſuitable diet and exerciſes, or only a temporary excitement, calculated for the particular occaſion? Alſo, whether perſons have ever thought of undergoing this proceſs, not for the purpoſe of running matches, but to recover health; with what ſucceſs

this has been done, and whether it is to be recommended for gout, corpulency, aſthma, nervous diſorder, or other maladies, as likely to be of ſervice?

The art muſt be of importance.

Theſe are queſtions, of the importance of which, thoſe who are beſt able to anſwer, may not be fully aware. But nothing which ſo ſuddenly changes the powers, and the very form and character of the body, from groſs to lean, from weakneſs to vigorous health, from a breathleſs and bloated carcaſe, to one active and untiring, can ever be unimportant, either to the art of phyſic in general, or to that branch of it more immediately connected with inquiries regarding health and longevity.

The queries to be put regarding jockies, running-horſes, or game-cocks, may be to the following effect:

1. *Jockies.*

Queries reſpecting Jockies.

1. What is the proceſs uſed in training them, and reducing their weight?

2. What effect has it upon their health and ſtrength?

3. What effect has it upon their mind, in regard to courage, quickneſs, &c.

4. How long do theſe effects continue?

5. After being reduced, do they quickly get fat again, or do they continue long in the ſtate to which they were brought?

6. Are jockies, accuſtomed to be thus treated, healthy and long lived?

2. *Running Horſes.*

Running horſes.

1. What are the principal objects to be attended to in regard to running-horſes? Do their perfections depend upon parentage, and whether moſt upon the male or the female? Is it neceſſary that the mare ſhould have gone her full time, to bring a perfect foal? Is the gradual growth of the foal eſſential? Is there a great difference, in regard to natural conſtitution, between horſes of the ſame parentage? What kind of form is in general preferred? Do you prefer great or ſmall bones? Which ſex is preferable for ſpeed, and which for ſtrength?

2. What is the beſt age for beginning to train horſes for the turf? Are they firſt put upon graſs? What is the effect of

ſoft meat? When ſhould they be put on hard meat? What are the effects thereof? Is it neceſſary to purge them frequently? Have the purges any tendency to weaken them? What food is reckoned the moſt nouriſhing? How often are they fed? What drinks are given them, and how often? Whether hot or cold? Is it neceſſary to keep their ſkin perfectly clean, and how? Is it neceſſary to make them perſpire much? What exerciſe is given them? How is the training completed?

3. After the training is completed, can the perfections thereby obtained be eaſily kept up? Does the proceſs effect merely a temporary change, or does it laſt during life? Are running horſes as long lived as others, or do they ſoon wear out?

3. *Game-Cocks.*

Game-cocks.

1. Does the ſuperiority of game-cocks depend upon parentage? Which is of moſt importance, the male or the female? Is it of any conſequence that the cock ſhould arrive rather gradually at maturity? Is there a great difference, in point of ſtrength and conſtitution, in game cocks of the ſame parentage? Do you prefer great or ſmall bones?

2. When do you begin to feed the young cocks? What diet and drink do you give them, and what is the proceſs by which they are brought to the greateſt poſſible height of ſtrength and ſpirit?

3. When the game-cocks are thus trained, how long do the effects thereof laſt? Are they temporary or permanent? Do game-cocks thus trained live ſhorter or longer than others of the ſame ſpecies?

4. What drugs are given to fighting-cocks immediately before the main begins? Is it not uſual, by giving them ſaffron, (or ſome drug which has the ſame effect with opium, as uſed among the Janiſaries, or brandy among the French ſoldiery,) to excite an unnatural and ſhort-lived courage? What are the effects of ſuch drugs? and how do they manage the feeding up to this point, ſo as to take advantage of this momentary excitement?

On the Dangers encountered in travelling over Downs, occaſioned by Quickſands, which are frequently found on the Sea Coaſt; with an Indication of the Means of avoiding them. By M. BIEMONTIER, *Inſpector-General of Bridges and Roads.* *

Quickſands formed by bodies of ſand tranſported by the wind into pools or water, where they form vaulted cavities till the hollow of the pool is filled up.

AFTER heavy and continued rains, there are formed at the edge of the ſea-downs, ſmall pools, or collections of water, frequently of ſeveral feet in depth. Strong winds diſlodge portions of ſand from the general maſs, and tranſport them to a diſtance; which falling in ſhowers on the clayey and ſheltered ſurface of theſe pools, deſcend gradually, and remain as it were in equilibrium in the midſt of the water, ſo as to form an infinity of little vaulted cavities. Theſe arches ſuſtain others, which are again ſurmounted in a ſimilar manner, till at length the maſs riſes, ſometimes to ſeveral feet above the level of the water; the ſurface becomes white and dry, and the ſnare lies perfectly concealed. Whoever walks over this ſtructure deſtroys the whole, the arches give way, and the intruder is immerſed ſometimes to his waiſt; but his alarm is uſually greater than the real danger; for if he were buried even up to the neck, he might eaſily extricate himſelf, only by retaining ſufficient preſence of mind not to ſtruggle, but to move ſlowly and deliberately; want of attention to this might hazard his deſtruction.

The ſurface is dry and ſcarcely differs in appearance from the contiguous ground, but it gives way when trod upon.

Management to avoid danger.

When the equilibrium of the maſſes of ſand is deſtroyed, they naturally fall into heaps, and it is only neceſſary that time ſhould be allowed for this to take place. When this has happened, the perſon immerſed ſhould gently lift up one leg, and remain in that poſition till the ſand has formed a ſufficient bottom to ſupport his raiſed foot; the other leg ſhould then be lifted up with the ſame precautions; and thus ſucceſſively, till he riſes to the ſurface. In the mean time, the water which had been confined in the hollows of the ſand will have alſo riſen, forming a pond (three or four inches deep) through which the adventurer may paſs in perfect ſafety.

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* Bibliotheque Phyſico-Economique, &c. de Sonini, November, 1805, page 186.

Animals when immerſed uſe the ſame method;

Cows, dogs, and other animals who frequent downs, an chance to fall into theſe quickſands, either through inſtinct o experience, make uſe of this method to regain their freedom provided, however, they be not too deeply immerſed to retai the free uſe of their ſhoulder joints, otherwiſe they cannot b extricated without aſſiſtance. I experienced this twice i one day; my horſe ſank to above the breaſt-leather, an although he was very ſtrong, his efforts to extricate himſel were unavailing, till we had removed ſo much of the ſan as impeded the action of thoſe joints.

but they are ſeldom caught in quickſands.

It rarely happens that animals accuſtomed to live on down are caught in theſe ſnares, which they are aware of, an know how to avoid.

Inſtance.

I attempted, but in vain, to force another horſe with the whip and ſpur into a quickſand; his owner, who acted a guide, aſſured me, that I ſhould not ſucceed, although there was no other indication of the ſpot than a flat ſurface, ſlightly wrinkled. By theſe marks the traveller may generally detect the concealed pitfall; but he may always avoid them by tracing the footſteps of the cattle, when viſible, or by walking a few fathoms above the bottom of the declivity, or on the ſummit of the down.

Another kind of quickſand,

Another kind of quickſand is ſometimes met with on the ſea-ſhore, between high and low water mark, which it is proper ſhould be here taken notice of. This is ſometimes the effect of rain, but more commonly of the ſea, when forced by wind and tempeſt beyond its uſual limits, which being generally more elevated than the diſtant land, the waters thus impelled forward are prevented from returning to their ancient bed; they therefore after forming in a body, drain away through the earth they have inundated, or brought with them, and form excavations beneath, large or ſmall, deep or ſhallow, according to circumſtances.

formed by the waters drained through the earth.

I ought not to omit a ſingular fact which paſſed under my own obſervation, and which ſeems to prove, (as I have already ſtated) that animals frequenting theſe plains, and living near the borders of the ſea, employ combined means, acquired undoubtedly by experience, to extricate themſelves from theſe cavities, wherein they muſt inevitably periſh, did they, as it appears natural they ſhould, attempt to eſcape by recoiling or by flight.

Traverſing the plain of Arcachon, after a violent tempeſt, which had been accompanied with heavy rains, we thought it prudent to get off our horſes and lead them by the bridle. One of the horſes who was left to himſelf, immediately quitted the company; and was retiring from the ſhore, but being compelled to return by the application of the whip, he went upon the quickſand, which probably he had attempted to avoid by deſertion; but the moment he felt the earth giving way, he crouched down, or rather threw himſelf precipitately on his ſide. The ground quickly ſank beneath and round about him; the water ſurmounted the ſand; the horſe was only wetted to the crupper, and we eſcaped with no other damage than the loſs of our ſtock of bread, which being ſoaked in the ſalt water was rendered unfit to be eaten.

Singular incident.

It may be received as matter of fact, that a man who ſhould experience a ſimilar misfortune, could not do better than to extend himſelf in the like manner, nearly in the attitude of a ſwimmer, when he throws himſelf into the water. It is ſcarcely neceſſary to explain the ſuperior advantage of this method; a plate of lead, of ſome thouſands of weight, and ſeveral feet in breadth, if caſt flat into any liquid body, would reach the bottom no quicker than the fluid could eſcape to make way for it; if a ſimilar body were to fall upon a quickſand, it would ſhake every part of it, but would prevent the ſand or earth from riſing, while the firm ſurrounding earth would confine it laterally: the ruins of the arched vaults would replace the waters which had been liberated from their ſubterraneous confinement; ſolid heaps would then neceſſarily be formed towards the centre, and the incumbent body would remain at the ſurface, or at leaſt it would not be ſwallowed up.

Travellers when caught in this ſnare ſhould throw themſelves down.

Theſe quickſands are generally denoted by ſmall ſtreams, below which, when practicable, there is no danger in paſſing.

Quickſands are denoted by rills of water.

X. Extract

X.

Extract from a Memoir by Meſſrs. FOURCROY *and* VAUQUELIN, *on the Guano, or Natural Manure, of the ſmall Iſlands of the South Sea, near the Coaſt of Peru.* *Read at the French National Inſtitute, by* A. LAUGIER.*

M. Humboldt the firſt who gave an account of the Guano.

AMONG the multitude of ſubjects worthy the attention of the naturaliſt, which the philoſophical Humboldt obſerved and collected during his travels, the Guano is not the leaſt conſiderable, from the intereſt which it excites. This celebrated naturaliſt, by making us acquainted with this ſingular matter, one of the principal reſources of agriculture in the countries he viſited, has given confirmation to a diſcovery made by the authors of this memoir, about the time of his return. Reading their memoir on the exiſtence of uric acid in the excrements of birds, it occurred to him that the Guano of the iſlets on the coaſt of Peru, which are frequented by great numbers of birds, might poſſibly be of the ſame nature. It remained for chemical inveſtigation to examine how far this conjecture was well founded; and Meſſrs. Fourcroy and Vauquelin undertook the analyſis of this matter. The following is the reſult of their labours, with this view, extracted from the Memoirs of the National Inſtitute.

Memoir by Meſſrs. Fourcroy and Vauquelin on the excrements of birds, ſuggeſted the notion that the Guano was derived from the ſame origin.

Before I enter upon a detail of the experiments made upon Guano, in order to aſcertain its nature, it may not be irrelevant to the ſubject to tranſcribe what M. Humboldt himſelf ſays of this ſubſtance in a note ſent to the authors of this memoir.

Extract from M. Humboldt's note.

Guano found on certain ſmall iſlands,

"The Guano is found in abundance in the South Sea, in the Chinche iſlands, near Piſco; and alſo on the more ſouthern coaſts and iſlets of Ilo, Iza, and Arica. The inhabitants of Chancay, who make Guano an object of their commerce, go to and return from the Chinche iſlands once in 20 days.—Each veſſel contains from 1500 to 2000 cubic feet. A vanega ſells at Chancay for 14 livres, and at Arica for 15 livres, Tournois.

—in beds 50 or 60 feet thick.

"Guano is dug from beds 50 or 60 feet thick; where it is worked like the bog-ore of iron. The iſlets where it is found are frequented by a multitude of birds, particularly of the ſpecies of Ardea and Phœnicopterus, who rooſt there every night: but the excrements of theſe birds have hardly formed, in three centuries, a layer of four or five lines in depth. Is then the Guano the effect of ſome convulſion of the globe, like pit-coal and foſſil wood? The fertility of the naturally ſterile ſoil of Peru is derived from the Guano, which has become a material article of commerce. Fifty little veſſels, called *Guaneras*, are conſtantly employed in fetching this manure, for the ſupply of the coaſt. Its effluvium may be ſmelled at the diſtance of a quarter of a league. The ſailors accuſtomed to this ſmell of ammonia, feel no inconvenience from it; but we could not approach it without being affected with continued fits of ſneezing.

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* Annales de Chimie, Vol. LVI. p. 258.

The place frequented by vaſt numbers of birds.

Sterile ſoil of Peru made fruitful by the Guano.

Veſſels employed to collect it.

It has a ſtrong odour of ammonia.

" Maize is the particular vegetable for which Guano forms an excellent manure. The Spaniards learned its uſe of the Indians: If too much be thrown upon the maize, the root is burned and deſtroyed. Guano is too acidifiable, and is therefore a manure containing hydruret of azote; whilſt all other manures are rather hydrurets of carbon."

Maize particularly benefited by Guano as a manure.

Guano is of a dirty yellow colour, rather inſipid to the taſte, but poſſeſſing a powerful odour, partaking of caſtor and of valerian. It turns black in the fire, and exhales a white ſmoke of an ammoniacal ſmell.

Its appearance.

Its ſolubility in water, particularly with potaſh, determined the operators as to the method they ſhould purſue in its analyſis. They treated it ſucceſſively with water, with potaſh, and with muriatic acid; each of which methods preſented many phenomena, as related in the following part of this paper, diveſted of the particular details of proceſs, which are too extenſive for an extract.

Partly ſoluble in water.

Ten grammes of this matter, after being repeatedly waſhed with large quantities of boiling water, were reduced to $5\frac{7}{16}$ grammes. The water had obtained a red colour, which it communicated to paper ſtained with turnſole.

The ſolution is acid.

In diſtillation, the water yielded ammonia during the whole operation. Twenty-four hours afterwards, it had depoſited a dirty yellow powder, poſſeſſing very little flavour, but with an odour of caſtor: On the ſurface was a cryſtalline pellicle, of the ſame colour with the depoſition.

The water yielded ammonia by diſtillation, and depoſited a yellow powder with a ſmell of caſtor.

The liquor, filtered and again evaporated, till reduced to 3 grammes, on cooling again, depoſited a fawn-coloured powder, ſimilar to the former, but in leſs quantity.

The powder, and the mother-water, which had held it in ſolution, were ſeparately examined.

Examination of the powder.

The powder offered the following properties:—It is a concrete and pulverulent ſubſtance, of a brilliant cryſtalline aſpect, and of a dull yellow colour. Before the blow-pipe it is conſumed entirely away, yielding a ſlight empyreumatic odour of ammonia and pruſſic acid. It is very little ſoluble in cold water; but abundantly ſo in warm water, to which it communicates its yellowiſh colour. This ſolution, though taſteleſs, ſtrongly reddens the tincture of turnſole, precipitates ſolutions of acetate of lead, and of nitrate of ſilver and mercury, in coloured flakes, which are readily and completely rediſſolved by nitric acid.

This matter inſtantaneouſly diſſolves in an alkaline ley, which it tinges of a deep brown colour, exhaling a pungent ſmell of ammonia. Sulphuric acid poured into the concentrated alkaline ſolution, throws down a very thick whitiſh precipitate, and diſengages a briſk odour, reſembling that of weak acetic acid.

It is an acidulous ſalt, compoſed of animal acid, ammonia, and lime.

The learned authors of this memoir conclude from their experiments, that this powder is an acidulous ſalt, compoſed of animal acid, ammonia, and a little lime. In fact, very weak nitric acid, wherein this ſalt had been macerated in order to diſengage the acid it contained, from its baſes, yielded, on evaporation, copious ammoniacal vapours, by the addition of potaſh, and unequivocal ſigns of the preſence of lime, by the addition of oxalic acid.

Analyſis of the powder when deprived of its ammonia and lime.

When thus deprived of its ammonia and lime, this matter is leſs coloured and leſs ſoluble than before. Its ſolution in boiling water depoſits pretty hard and brilliant cryſtals, and more deeply reddens turnſole paper. It combines readily, and without any ammoniacal vapour, with potaſh, from which all the acids again ſeparate it. Heat turns it black; and it burns, without leaving any reſiduum, with an odour of ammonia and of pruſſic acid. A neutral combination of it with ammonia will not precipitate the ſolution of ſulphate of alumine, as is done by lithic acid.

From theſe facts it appears evident, 1, that the matter taken up by the boiling water from Guano is an acid, partly ſaturated with ammonia and a little lime; 2, that this acid is an animal product, becauſe it yields ammonia and pruſſic acid, when decompoſed by fire; 3, that the ſame acid, according to all the known properties, muſt be uric acid, ſimilar to that contained in the excrements of aquatic birds; 4, that it forms about one fourth part of the Guano.

The acid of Guano is uric acid, forming about ¼ of the whole.

The mother-water which depoſited the powder, whoſe qualities have been juſt examined, is very acid; potaſh cauſes a copious diſengagement of ammonia: It contains, therefore, an ammoniacal ſalt. Nitrate of barytes and of ſilver announce the preſence of muriatic and ſulphuric ſalts; which are precipitated in white flakes by lime-water, and are re-diſſolved, though with difficulty, in muriatic acid.

Analyſis of the mother-water which depoſited the powder. It contains an ammoniacal ſalt,

This precipitate cauſed by lime water, is evidently formed of two ſalts, both ſoluble in acids without effervescence; one eaſily, and without the aſſiſtance of heat, the other with difficulty, even with the aid of heat; the former reſiſts calcination, the latter is decompoſed by fire, and afterwards diſſolves in acids with effervescence. The firſt is phoſphate of lime, the ſecond oxalate of lime.

—and phoſphate and oxalate of lime,

Meſſrs. Fourcroy and Vauquelin wiſhed to ſeparate theſe two ſalts, without their undergoing any alteration; and with this view they made uſe of weak nitric acid, which diſſolved the phoſphate of lime, and left the oxalate untouched. The latter ſalt, on being treated with a ſolution of carbonate of potaſh, yielded a precipitate that diſſolved with effervescence in nitric acid: This ſolution diſplayed all the properties of nitrate of lime. The acid ſeparated from the lime was taken up by the potaſh: in fact, the liquor poſſeſſed the characters of oxalate of potaſh; it precipitated with lime-water, a very divided powder, with ſulphate of lime, in flakes, which would not readily unite; and with all the metallic ſolutions capable of precipitation by oxalic acid. Sulphate of alumine cauſed no precipitate, as it would have done with honiſtate of potaſh.

The potaſh found in the mother-water, after its precipitation by lime-water, and the diſengagement of ammonia, cauſed by the addition of potaſh to the mother-water, prior to its decompoſition by lime-water, ſufficiently ſhew that theſe two alkalis ſaturate the acids contained in the mother-water of Guano;

The mother-water contains oxalates, phoſphates, ſulphates, and muriates of potaſh, and of ammonia.

Guano; and that the mother-water certainly contains oxalates, phoſphates, ſulphates, and muriates of potaſh, and of ammonia.

The Guano left from the firſt waſhing,

The five grammes and ſeven-tenths, left after the waſhing of the ten grammes originally taken for analyſis, were treated with cauſtic potaſh, which took up eight-tenths. This alkaline ſolution contained only uric acid, and a ſmall portion of fat matter.

—contains uric acid.

Phoſphate of lime, iron, and carbonate of lime;

The 4.9 grammes left by the cauſtic potaſh, were treated with muriatic acid: the product was phoſphate of lime, iron, and an atom of carbonate of lime.

—and left quartzoſe and ferruginous ſand.

After theſe applications of water, of cauſtic potaſh, and of muriatic acid, there remained of the 10 grammes of Guano, only 3.1 grammes of matter, compoſed of quartzoſe and ferruginous ſands.

Recapitulation of component parts.

From the foregoing intereſting analyſis, it appears that the manure of the iſlets of the South Sea is formed of,—

1. Uric acid to the amount of $\frac{1}{4}$ of the whole compound; partly ſaturated with ammonia and lime:

2. Oxalic acid, partly ſaturated with ammonia and potaſh:

3. Phoſphoric acid, combined with the ſame baſes and with lime:

4. Small quantities of ſulphate and muriates of potaſh and ammonia:

5. A ſmall portion of fat matter:

6. Sand, partly quartzoſe and partly ferruginous.

Remarks.

The exiſtence of Guano in places frequented by vaſt numbers of birds, and the identity of its nature with that of the excrements of aquatic birds, neceſſarily throw conſiderable light on the origin of this matter.

The analyſis proves how well founded was the ingenious compariſon of the learned naturaliſt, to whom we are indebted for our knowledge of this ſubſtance, no leſs intereſting to us than uſeful to the inhabitants of Peru. It confirms the important diſcovery made by the reſearches of Meſſrs. Fourcroy and Vauquelin. In a word, this analyſis poſſeſſes the advantage of proving a well-known maxim, that the ſciences mutually enrich and enlighten each other with the light they poſſeſs; and it affords a new occaſion to remark that among the ſciences, there are perhaps none which have ſo immediate and ſo neceſſary a connection as Chemiſtry and Natural Hiſtory.

XI.

Note on a Varniſh for glazing Cups. By M. PARMENTIER *.

Account of very light coffee-cups finely varniſhed.

M. BOMPOIX, chief apothecary to the French Military Hoſpital at Genoa, having ſent me ſome coffee-cups of a remarkable lightneſs, and glazed with a varniſh which is held in great repute, perhaps only becauſe its preparation is kept a ſecret in that country; I requeſted him to uſe his utmoſt endeavours to procure me the receipt. He obtained it through the medium of one of his pupils, who learned the ſecret from the artificer at the manufactory, and had made from his preſcription a varniſh in every reſpect equal to that in queſtion:

Receipt for the varniſh.

It conſiſted of lintſeed oil 1 ½ lbs.; amber 1lb.; litharge in powder, minium in powder, ceruſe in powder, each, 5 oz.

Boil the lintſeed oil in an unglazed earthen veſſel, and tie the litharge, minium, and ceruſe in a linen bag, which is to be ſuſpended in the oil whilſt boiling, ſo that it may not touch the bottom of the veſſel. When the oil begins to turn brown, take out the bag, and put in a clove of garlic, cleared of the ſkin; continue the boiling; and when the garlic is dried away, put in another and another, to the amount of ſix or ſeven. In the mean time, the amber is to be melted in another unglazed veſſel, according to the method hereafter preſcribed; and when the oil has been ſufficiently boiled, the fuſed amber is to be poured into it.

To melt the Amber.

Fuſion of the amber.

Take two ounces of lintſeed oil, to ſoften the amber and to aſſiſt its fuſion by a very briſk fire, and when the amber is melted, add the lintſeed oil, and boil the whole about two minutes. The fluid muſt then be ſtrained through a coarſe cloth, and when cold put into a bottle well corked, to prevent from drying.

Method of uſing the Varniſh.

Application of the varniſh.

Let the piece intended to be varniſhed be firſt well poliſhed, and then apply the varniſh in the following manner:

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* Annales de Chimie, Vol. LVI. p. 254.

Mix lamp black with varniſh and a little turpentine, with a hair pencil, and lay one coat on the piece; when this is dry, lay on another, and repeat the proceſs till four coats have been laid on, taking care to let each dry before the application of the next. When the laſt is dry, put the piece into a ſtove or oven to complete the drying, and then poliſh it with pumice and Tripoli powder.

Method of preparing the Piece intended to be varniſhed.

Manner of making the wooden cups.

Make the cups of hazel, alder, or cherry-tree, which are preferable to other woods for this uſe, becauſe they are porous when perfectly dry, and do not warp. Form them according to fancy, and dry them in an oven. The work muſt be poliſhed as if it were complete; and afterwards lay on the varniſh as already preſcribed.

Red varniſh.

If it ſhould be wiſhed to give a red ground to the article, mix a little minium, or rather cinabar, with the varniſh. Any other colour may in like manner be mixed with it, as may beſt pleaſe the fancy of the operator.

XII.

*Account of a Series of Experiments, ſhewing the Effects of Compreſſion in modifying the Action of Heat**. *By* SIR JAMES HALL, *Bart. F. R. S. Edinburgh.*

SECTION I.

Ancient Revolutions of the Mineral Kingdom.—Vain Attempts to explain them.—Dependance of Geology on Chemiſtry.—Importance of the Carbonate of Lime.—Dr BLACK'S *Diſcovery of Carbonic Acid ſubverted the former theories depending on Fire, but gave Birth to that of Dr.* HUTTON.*—Progreſs of the Author's Ideas with Regard to that Theory.—Experiments with Heat and Compreſſion, ſuggeſted to Dr.* HUTTON *in* 1790.*—Undertaken by the Author in* 1798.*—Speculations on which his Hopes of Succeſs were founded.*

Violent revolutions of the ſurface of the globe.

HOEVER has attended to the ſtructure of rocks and mountains, muſt be convinced, that our globe has not always

exiſted

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* The highly intereſting experiments of Sir James Hall upon the effects of heat modified by compreſſion, were communicated to the

-duced in its preſent ſtate; but that every part of its maſs, ſo far at leaſt as our obſervations reach, has been agitated and ſubverted by the moſt violent revolutions.

Geological ſyſtems imperfect.

Facts leading to ſuch ſtriking concluſions, however imperfectly obſerved, could not fail to awaken curioſity, and give riſe to a deſire of tracing the hiſtory, and of inveſtigating the cauſes, of ſuch ſtupendous events; and various attempts were made in this way, but with little ſucceſs; for while diſcoveries of the utmoſt importance and accuracy were made in aſtronomy and natural philoſophy, the ſyſtems produced by the Geologiſts were ſo fanciful and puerile, as ſcarcely to deſerve a ſerious refutation.

becauſe chemical knowledge was in its infancy.

One principal cauſe of this failure ſeems to have lain in the very imperfect ſtate of chemiſtry, which has only of late years begun to deſerve the name of a ſcience. While chemiſtry was in its infancy, it was impoſſible that geology ſhould make any progreſs; ſince ſeveral of the moſt important circumſtances to be accounted for by this latter ſcience, are admitted on all hands to depend upon principles of the former. The conſolidation of looſe ſand into ſtrata of ſolid rock; the cryſtalline arrangement of ſubſtances accompanying thoſe ſtrata, and blended with them in various modes, are circumſtances of a

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the Royal Society of Edinburgh in Auguſt 1804, and were tranſmitted to our Journal by the author in the following month. They appear in Vol. IX. page 98. That conciſe narrative could not but ſtrongly excite the curioſity of philoſophers and geologiſts, and direct their earneſt expectations to a fuller detail In the laſt ſeſſion, June 3, 1805, a very ample communication was made, which has been printed with five quarto plates, very beautifully engraved by Lizars, from deſigns by Sir James. I cannot but conſider it as one of thoſe high marks of approbation, with which the Philoſophical Journal has been honoured from time to time, that the author has again directed his attention to this periodical work, as the vehicle through which his diſcoveries ſhould be more extenſively circulated. With this view he has not only favoured me with the memoir as ſoon as completed, but has liberally taken upon himſelf the expence of engraving the plates for the Journal in the ſame ſuperior ſtyle. By this means the numbers containing his memoir will be enriched with ten additional plates beſides thoſe uſually given:—for I ſhall with great ſatisfaction follow the ſteps of the worthy baronet by preſenting the additional expences of paper and print to the reader without charge. W. N.

chemical nature, which all thoſe who have attempted to frame theories of the earth have endeavoured by chemical reaſonings to reconcile to their hypotheſes.

Fire and water adduced as the agents in two theories.

Fire and *water*, the only agents in nature by which ſtony ſubſtances are produced, under our obſervation, were employed by contending ſects of geologiſts, to explain all the phenomena of the mineral kingdom.

Water has little agency on minerals.

But the known properties of water are quite repugnant to the belief of its univerſal influence, ſince a very great proportion of the ſubſtances under conſideration are inſoluble, or nearly ſo, in that fluid; and ſince, if they were all extremely ſoluble, the quantity of water which is known to exiſt, or that could poſſibly exiſt in our planet, would be far too ſmall to accompliſh the office aſſigned to it in the Neptunian theory *.

Common fire does not explain the facts.

On the other hand, the known properties of fire are no leſs inadequate to the purpoſe; for, various ſubſtances which frequently occur in the mineral kingdom, ſeem, by their preſence, to preclude its ſuppoſed agency; ſince experiment ſhews, that, in our fires, they are totally changed or deſtroyed.

Hence both theories were doubtful.

Under ſuch circumſtances, the advocates of either element were enabled, very ſucceſsfully, to refute the opinions of their adverſaries, though they could but feebly defend their own: and, owing, perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both ſyſtems maintained a certain degree of credit; and writers on geology indulged themſelves, with a ſort of impunity, in a ſtyle of unphiloſophical reaſoning, which would not have been tolerated in other ſciences.

Carbonate of lime is of extenſive importance,

Of all mineral ſubſtances, the *carbonate of lime* is unqueſtionably the moſt important in a general view. As limeſtone or marble, it conſtitutes a very conſiderable part of the ſolid maſs of many countries; and, in the form of veins and nodules of ſpar, pervades every ſpecies of ſtone. Its hiſtory is thus interwoven in ſuch a manner with that of the mineral kingdom at large, that the fate of any geological theory muſt very much depend upon its ſucceſsful application to the various conditions of this ſubſtance. But, till Dr. Black, by his diſcovery of carbonic acid, explained the chemical nature of the carbonate, no rational theory could be formed, of the chemical revolutions which it has undoubtedly undergone.

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* *Illuſtrations of the Huttonian Theory*, by Mr. Profeſſor Playfair, § 430.

ſeems not producible by heat.

This diſcovery was, in the firſt inſtance, hoſtile to the ſuppoſed action of fire; for the decompoſition of limeſtone by fire in every common kiln being thus proved, it ſeemed abſurd to aſcribe to that ſame agent the formation of limeſtone, or of any maſs containing it.

Dr. Hutton's theory.

The contemplation of this difficulty led Dr. Hutton to view the action of fire in a manner peculiar to himſelf, and thus to form a geological theory, by which, in my opinion, he has furniſhed the world with the true ſolution of one of the moſt intereſting problems that has ever engaged the attention of men of ſcience.

He ſuppoſed,

That rocks have undergone heat under ſtrong preſſure.

I. That heat has acted, at ſome remote period, on all rocks.

II. That during the action of heat, all theſe rocks (even ſuch as now appear at the ſurface) lay covered by a ſuperincumbent maſs, of great weight and ſtrength.

III. That in conſequence of the combined action of heat and preſſure, effects were produced different from thoſe of heat on common occaſions; in particular, that the carbonate of lime was reduced to a ſtate of fuſion, more or leſs complete, without any calcination.

The eſſential and characteriſtic principle of his theory is thus compriſed in the word *compreſſion*; and by one bold hypotheſis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for thoſe circumſtances in which minerals are found to differ from the uſual products of our furnaces.

Singular contraſt of the perſpicuity of Dr. Hutton's converſation, and the obſcurity of his writings.

This ſyſtem, however, involves ſo many ſuppoſitions, apparently in contradiction to common experience, which meet us on the very threſhold, that moſt men have hitherto been deterred from the inveſtigation of its principles, and only a few individuals have juſtly appreciated its merits. It was long before I belonged to the latter claſs; for I muſt own, that, on reading Dr. Hutton's firſt geological publication, I was induced to reject his ſyſtem entirely, and ſhould probably have continued ſtill to do ſo, with the great majority of the world, but for my habits of intimacy with the author; the vivacity and perſpicuity of whoſe converſation formed a ſtriking contraſt to the obſcurity of his writings. I was induced by that charm, and by the numerous original facts which his ſyſtem had led him to obſerve, to liſten to his arguments, in favour of opinions which I then looked upon as viſionary. I thus derived from his converſation the ſame advantage which the world has lately done from the publication of Mr. Playfair's *Illuſtrations;* and, experienced the ſame influence which is now exerted by that work, on the minds of our moſt eminent men of ſcience.

The author' progreſs in the Doctor's theory.

After three years of almoſt daily warfare with Dr. Hutton, on the ſubject of his theory, I began to view his fundamental principles with leſs and leſs repugnance. There is a period, I believe, in all ſcientific inveſtigations, when the conjectures of genius ceaſe to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, againſt its improbability as an hypotheſis: The partial view which we then obtain of truth, is perhaps the moſt attractive of any, and moſt powerfully ſtimulates the exertions of an active mind. The miſt which obſcured ſome objects diſſipates by degrees, and allows them to appear in their true colours; at the ſame time, a diſtant proſpect opens to our view, of ſcenes unſuſpected before.

He propoſes experimental confirmation,

rejected by the Doctor.

Entering now ſeriouſly into the train of reaſoning followed by Dr. Hutton, I conceived that the chemical effects aſcribed by him to compreſſion, ought, in the firſt place, to be inveſtigated; for, unleſs ſome good reaſon were given us for believing that heat would be modified by preſſure, in the manner alledged, it would avail us little to know that they had acted together. He reſted his belief of this influence on analogy; and on the ſatisfactory ſolution of all the phenomena furniſhed by this ſuppoſition. It occurred to me, however, that this principle was ſuſceptible of being eſtabliſhed in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this propoſal, on account of the immenſity of the natural agents, whoſe operations he ſuppoſed to lie far beyond the reach of our imitation; and he ſeemed to imagine, that any ſuch attempt muſt undoubtedly fail, and thus throw diſcredit on opinions already ſufficiently eſtabliſhed, as he conceived, on other principles. I was far, however, from being convinced by theſe arguments; for, without being able to prove that any artificial compreſſion to which we could expoſe the carbonate, would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might poſſibly perform all that was hypothetically aſſumed in the Huttonian theory. On the other hand, I conſidered myſelf as bound, in practice, to pay deference to his opinion, in a field which he had already ſo nobly occupied; and abſtained, during the remainder of his life, from the proſecution of ſome experiments with compreſſion, which I had begun in 1790.

Experimental inveſtigation undertaken.

In 1798, I reſumed the ſubject with eagerneſs, being ſtill of opinion that the chemical law which forms the baſis of the Huttonian theory, ought, in the firſt place, to be inveſtigated experimentally; all my ſubſequent reflections and obſervations having tended to confirm my idea of the importance of this purſuit, without in any degree rendering me more apprehenſive as to the reſult.

Order of the preſent treatiſe.

In the arrangement of the following paper, I ſhall firſt confine myſelf to the inveſtigation of the chemical effects of heat and compreſſion, reſerving to the concluding part the application of my reſults to Geology. I ſhall then appeal to the volcanoes, and ſhall endeavour to vindicate the laws of action aſſumed in the Huttonian theory, by ſhewing, that lavas, previous to their eruptions, are ſubject to ſimilar laws; and that the volcanoes, by their ſubterranean and ſubmarine exertions, muſt produce, in our times, reſults ſimilar to thoſe aſcribed, in that theory, to the former action of fire.

In comparing the Huttonian operations with thoſe of the volcanoes, I ſhall avail myſelf of ſome facts, brought to light in the courſe of the following inveſtigations, by which a preciſe limit is aſſigned to the intenſity of the heat, and to the force of compreſſion, required to fulfil the conditions of Dr. Hutton's hypotheſis: For, according to him, the power of thoſe agents was very great, but quite indefinite; it was therefore impoſſible to compare their ſuppoſed effects in any preciſe manner with the phenomena of nature.

Argument reſpecting carbonate of lime.

My attention was almoſt excluſively confined to the carbonate of lime, about which I reaſoned as follows: The carbonic acid, when uncombined with any other ſubſtance, exiſts naturally in a gaſeous form, at the common temperature of our atmoſphere; but when in union with lime, its volatility is repreſſed, in that ſame temperature, by the chemical force of the earthy ſubſtance, which retains it in a ſolid form. When the temperature is raiſed to a full red-heat, the acid acquires a volatility by which that force is overcome, it eſcapes from the lime, and aſſumes its gaſeous form. It is evident, that were the attractive force of the lime increaſed, or the volatility of the acid diminiſhed by any means, the compound would be enabled to bear a higher heat without decompoſition, than it can in the preſent ſtate of things. Now, preſſure muſt produce an effect of this kind; for when a mechanical force oppoſes the expanſion of the acid, its volatility muſt, to a certain degree, be diminiſhed. Under preſſure, then, the carbonate may be expected to remain unchanged in a heat, by which, in the open air, it would have been calcined. But experiment alone can teach us what compreſſing force is requiſite to enable it to reſiſt any given elevation of temperature; and what is to be the reſult of ſuch an operation. Some of the compounds of lime with acids are fuſible, others refractory; the carbonate, when conſtrained by preſſure to endure a proper heat, may be as fuſible as the muriate.

Preſſure muſt oppoſe the expanſion and eſcape of the acid, and enable it to ſupport a ſtronger heat.

Probability that the carbonate might not be of difficult fuſion.

Facts which indicate its melting heat.

One circumſtance, derived from the Huttonian Theory, induced me to hope, that the carbonate was eaſily fuſible, and indicated a preciſe point, under which that fuſion ought to be expected. Nothing is more common than to meet with nodules of calcareous ſpar incloſed in whinſtone; and we ſuppoſe, according to the Huttonian theory, that the whin and the ſpar had been liquid together; the two fluids keeping ſeparate, like oil and water. It is natural, at the junction of theſe two, to look for indications of their relative fuſibilities; and we find, accordingly, that the termination of the ſpar is generally globular and ſmooth; which ſeems to prove, that, when the whin became ſolid, the ſpar was ſtill in a liquid ſtate; for had the ſpar congealed firſt, the tendency which it ſhews, on all occaſions of freedom, to ſhoot out into prominent cryſtals, would have made it dart into the liquid whin, according to the peculiar forms of its cryſtallization; as has happened with the various ſubſtances contained in whin, much more refractory than itſelf, namely, augite, felſpar, &c.; all of which having congealed in the liquid whin, have aſſumed their peculiar forms with perfect regularity. From this I concluded, that when the whin congealed, which muſt have happened about 28° or 30° of Wedgwood, the ſpar was ſtill liquid. I therefore expected, if I could compel the carbonate to bear a heat of 28° without decompoſition, that it would enter into fuſion. The ſequel will ſhew that this conjecture was not without foundation.

The experiments introduced.

I ſhall now enter upon the deſcription of thoſe experiments, the reſult of which I had the honour to lay before this Society on the 30th of Auguſt laſt (1804); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various particulars, to ſteer between the oppoſite faults of prolixity and barrenneſs. My object ſhall be to deſcribe, as ſhortly as poſſible, all the methods followed, ſo as to enable any chemiſt to repeat the experiments; and to dwell particularly on ſuch circumſtances only as ſeem to lead to concluſions of importance.

The reſult being already known, I conſider the account I am about to give of the execution of theſe experiments, as addreſſed to thoſe who take a particular intereſt in the progreſs of chemical operations: in the eyes of ſuch gentlemen, I truſt, that none of the details into which I muſt enter, will appear ſuperfluous.

SECTION II.

Principle of Execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the Muzzle.—Method with the fuſible Metal.—Remarkable Effects of its Expanſion.—Neceſſity of introducing Air.—Reſults obtained.

The author's early contrivances for confining elaſtic ſubſtances at high temperatures.

When I firſt undertook to make experiments with heat acting under compreſſion, I employed myſelf in contriving various devices of ſcrews, of bolts, and of lids, ſo adjuſted, I hoped, as to confine all elaſtic ſubſtances; and perhaps ſome of them might have anſwered. But I laid aſide all ſuch devices, in favour of one which occurred to me in January 1798; which, by its ſimplicity, was of eaſy application in all caſes, and accompliſhed all that could be done by any device, ſince it ſecured perfect ſtrength and tightneſs to the utmoſt that the veſſels employed could bear, whether formed of metallic or earthy ſubſtance. The device depends upon the following general view: If we take a hollow tube or barrel (AD *Pl.* ix. fig.

The method adopted ultimately was to include the ſubject in an iron barrel, and cloſe the aperture by fuſion;

fig. 1.)[*] cloſed at one end, and open at the other, of one foot or more in length; it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if neceſſary, expoſed to extreme cold. If, then, the ſubſtance which we mean to ſubject to the combined action of heat and preſſure be introduced into the breech or cloſed end of the barrel (CD), and if the middle part be filled with ſome refractory ſubſtance, leaving a ſmall empty ſpace at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the ſubject of experiment, is kept cool, and thus cloſe the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of ſealing-wax. Things being then reverſed, and the breech put into the furnace, a heat of any required intenſity may be applied to the ſubject of experiment, now in a ſtate of conſtraint.

Firſt experiment with the muzzle plugged and welded,

My firſt application of this ſcheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very ſtrongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previouſly rammed into a cartridge of paper or paſteboard, in order to protect it from the iron, by which, in ſome former trials, the ſubject of experiment had been contaminated throughout during the action of heat. I then rammed the reſt of the barrel full of pounded clay previouſly baked in a ſtrong heat, and I had the muzzle cloſed like the breech, by a plug of iron welded upon it in a common forge; the reſt of the barrel being kept cold during this operation, by means of wet cloths. The breech of the barrel was then put horizontally into a common muffle, heated to about 25° of Wedgwood. To the muzzle a rope was fixed, in ſuch a manner, that the barrel could be withdrawn without danger from an exploſion [†]. I likewiſe, about this time cloſed the muzzle of

in another inſtance ſoldered.

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[*] This plate will be given in No. 54, being the ſupplement to the preſent volume.

[†] On one occaſion, the importance of this precaution was ſtrongly felt. Having inadvertently introduced a conſiderable quantity of moiſture into a welded barrel, an exploſion took place, before the heat had riſen to redneſs, by which, part of the barrel was ſpread out to a flat plate, and the furnace was blown to pieces. Dr. Kennedy, who happened to be preſent on this occaſion, obſerved, that notwithſtanding this accident, the time might come when we ſhould employ water in theſe experiments to aſſiſt the force

of the barrel, by means of a plug, fixed by ſolder only; which method had this peculiar advantage, that I could ſhut and open the barrel without having recourſe to a workman. In theſe trials, though many barrels yielded to the expanſive force, others reſiſted it, and afforded ſome reſults that were in the higheſt degree encouraging, and even ſatisfactory, could they have been obtained with certainty on repetition of the proceſs. In many of them, chalk, or common limeſtone previouſly pulveriſed, was agglutinated into a ſtony maſs, which required a ſmart blow of a hammer to break it, and felt under the knife like a common limeſtone; at the ſame time, the ſubſtance, when thrown into nitric acid, diſſolved entirely with violent efferveſcence.

Satisfactory reſults.

In one of theſe experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been uſed to fill the barrel, was ſtained black throughout, to the diſtance of two-thirds of the length of the barrel from its breech. This circumſtance is of importance, by ſhewing, that though all is tight at the muzzle, a protruſion may take place along the barrel, greatly to the detriment of complete compreſſion: and, at the ſame time, it illuſtrates what has happened occaſionally in nature, where the bituminous matter ſeems to have been driven by ſuperior local heat, from one part of a coaly bed, though retained in others, under the ſame compreſſion. The bitumen ſo driven off being found, in other caſes, to pervade and tinge beds of ſlate and of ſandſtone.

Volatile matter may be driven from one to another part of a cloſed barrel.

I was employed in this purſuit in ſpring 1800, when an event of importance interrupted my experiments for about a year. But I reſumed them in March 1801, with many new plans of execution, and with conſiderable addition to my apparatus.

In the courſe of my firſt trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemiſts, that a certain compoſition of

Experiments in which the fuſible metal was uſed as the plug.

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force of compreſſion. I have ſince made great uſe of this valuable ſuggeſtion: but he ſcarcely lived, alas! to ſee its application; for my firſt ſucceſs in this way took place during his laſt illneſs.—I have been expoſed to no riſk in any other experiment with iron barrels; matters being ſo arranged, that the ſtrain againſt them has only commenced in a red heat, in which the metal has been ſo far ſoftened, as to yield by laceration like a piece of leather.

different metals*, produces a ſubſtance ſo fuſible, as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and diſpatch, might be gained in theſe experiments, by ſubſtituting this metal for the baked clay above mentioned: That after introducing the carbonate into the breech of the barrel, the fuſible metal, in a liquid ſtate, might be poured in, ſo as to fill the barrel to its brim: That when the metal had cooled and become ſolid, the breech might, as before, be introduced into a muffle, and expoſed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fuſible metal being melted but what lay at the breech, the reſt, continuing in a ſolid ſtate, would effectually confine the carbonic acid: That after the action of ſtrong heat had ceaſed, and after all had been allowed to cool completely, the fuſible metal might be removed entirely from the barrel, by means of a heat little above that of boiling water, and far too low to occaſion any decompoſition of the carbonate by calcination, though acting upon it in freedom; and then, that the ſubject of experiment might, as before, be taken out of the barrel.

Advantages of this method.

This ſcheme, with various modifications and additions, which practice has ſuggeſted, forms the baſis of moſt of the following methods.

A ſtriking phenomenon. When the barrel was completely filled with fuſible metal only, and the cloſed end of the iron expoſed to heat in a muffle, the greater expanſion of the fluid forced it through the texture of the iron in very fine wire reſembling wool.

In the firſt trial, a ſtriking phenomenon occurred, which gave riſe to the moſt important of theſe modifications. Having filled a gun-barrel with the fuſible metal, without any carbonate; and having placed the breech in a muffle, I was ſurpriſed to ſee, as the heat approached to redneſs, the liquid metal exuding through the iron in innumerable minute drops, diſperſed all round the barrel. As the heat advanced, this exudation increaſed, till at laſt the metal flowed out in continued ſtreams, and the barrel was quite deſtroyed. On ſeveral occaſions of the ſame kind, the fuſible metal, being forced through ſome very minute aperture in the barrel, ſpouted from it to the diſtance of ſeveral yards, depoſiting upon any ſubſtance oppoſed to the ſtream, a beautiful aſſemblage of fine wire, exactly in the form of wool. I immediately underſtood that the phenomenon was produced by the ſuperior expanſion of the liquid over the ſolid metal, in conſequence of which, the fuſible metal was driven through the iron as water was driven through ſilver * by mechanical percuſſion in the Florentine experiment. It occurred to me, that this might be prevented by confining along with the fuſible metal a ſmall quantity of air, which, by yielding a little to the expanſion of the liquid, would ſave the barrel. This remedy was found to anſwer completely, and was applied, in all the experiments made at this time †.

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* Eight parts of biſmuth, five of lead, and three of tin.

Remedy. A ſmall quantity of air was left in the barrel.

I now propoſed, in order to keep the carbonate clean, to incloſe it in a ſmall veſſel; and to obviate the difficulty of removing the reſult at the concluſion of the experiment, I further propoſed to connect that veſſel with an iron ramrod, longer than the barrel, by which it could be introduced or withdrawn at pleaſure.

The carbonate was incloſed in a ſmall ſeparate veſſel.

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[*] *Eſſays of Natural Experiments made in the Academie del Cimento,* tranſlated by Waller, London, 1684, page 117. The ſame in Muſſchenbroek's Latin Tranſlation, Ludg. Bat. 1731, p. 63.

[†] I found it a matter of much difficulty to aſcertain the proper quantity of air which ought to be thus incloſed. When the quantity was too great, the reſult was injured by diminution of elaſticity, as I ſhall have occaſion fully to ſhew hereafter. When too ſmall, or when, by any accident, the whole of this included air was allowed to eſcape, the barrel was deſtroyed.

I hoped to aſcertain the bulk of air neceſſary to give liberty to the expanſion of the liquid metal, by meaſuring the actual quantity expelled by known heats from an open barrel filled with it. But I was ſurpriſed to find, that the quantity thus diſcharged, exceeded in bulk that of the air which, in the ſame heats, I had confined along with the carbonate and fuſible metal in many ſucceſsful experiments. As the expanſion of the liquid does not ſeem capable of ſenſible diminution by an oppoſing force, this fact can only be accounted for by a diſtenſion of the barrel. In theſe experiments, then, the expanſive force of the carbonic acid, of the included air, and of the fuſible metal, acted in combination againſt the barrel, and were yielded to in part by the diſtenſion of the barrel, and by the condenſation of the included air. My object was to increaſe the force of this mutual action, by diminiſhing the quantity of air, and by other devices to be mentioned hereafter. Where ſo many forces were concerned, the laws of whoſe variations were unknown, much preciſion could not be expected, nor is it wonderful, that in attempting to carry the compreſſing force to the utmoſt, I ſhould have deſtroyed barrels innumerable.

Deſcription of this apparat

A ſmall tube of glaſs,* or of Reaumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length, (fig. 2, A) was half filled with pounded carbonate of lime, rammed as hard as poſſible; the other half of the tube being filled with pounded ſilex, or with whatever occurred as moſt likely to prevent the intruſion of the fuſible metal in its liquid and penetrating ſtate. This tube ſo filled, was placed in a frame or cradle of iron (*d f k h*, figs. 3, 4, 5, and 6), fixed to the end (*m*) of a ram-rod (*m n*). The cradle was from ſix to three inches in length, and as much in diameter as a gun-barrel would admit with eaſe. It was compoſed of two circular plates of iron, (*d e f g* and *h i k l*, ſeen edgewiſe in the figures), placed at right-angles to the ram-rod, one of theſe plates (*d e f g*) being fixed to it by the centre (*m*). Theſe plates were connected together by four ribs or flattened wires of iron (*d h*, *e i*, *f k*, and *g l*,) which formed the cradle into which the tube (A), containing the carbonate, was introduced by thruſting the adjacent ribs aſunder. Along with the tube juſt mentioned, was introduced another tube (B), of iron or porcelain, filled only with air. Likewiſe, in the cradle, a pyrometer† piece (C) was placed in contact with (A) the tube containing the carbonate. Theſe articles generally occupied the whole cradle; when any ſpace remained, it was filled up by a piece of chalk dreſſed for the purpoſe. (Fig. 4, repreſents the cradle filled, as juſt deſcribed).

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* I have ſince conſtantly uſed tubes of common porcelain, finding glaſs much too fuſible for this purpoſe.

† The pyrometer-pieces uſed in theſe experiments were made, under my own eye. Neceſſity compelled me to undertake this laborious and difficult work, in which I have already ſo far ſucceeded as to obtain a ſet of pieces, which, though far from complete, anſwer my purpoſe tolerably well. I had lately an opportunity of comparing my ſet with that of Mr. Wedgwood, at various temperatures, in furnaces of great ſize and ſteadineſs. The reſult has proved, that my pieces agree as well with each other as his, though with my ſet each temperature is indicated by a different degree of the ſcale. I have thus been enabled to conſtruct a table, by which my obſervations have been corrected, ſo that the temperatures mentioned in this paper are ſuch as would have been indicated by Mr. Wedgwood's pieces. By Mr. Wedgwood's pieces, I mean thoſe of the only ſet which has been ſold to the public, and by which the melting heat of pure ſilver is indicated at the 22d degree. I am well aware, that the late Mr. Wedgwood, in his Table of Fuſibilities, has ſtated that fuſion as taking place

Method of uſing the ſame,

Things being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fuſible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, ſo that the carbonate was placed very near the breech, (as repreſented in fig. 5, the fuſible metal ſtanding at *o*). The air-tube (B) being placed ſo as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though ſome of it might be driven off by the heat, ſo as to eſcape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held faſt. The barrel being then filled to the brim with fuſible metal, the apparatus was ready for the application of heat to the breech, (as ſhewn in fig. 6.) Plate X.

the furnace and muffle arrangement, &c.

In the experiments made at this time, I uſed a ſquare brick furnace, (figs. 7 and 8, having a muffle (*r s*) traverſing it horizontally and open at both ends. This muffle being ſupported in the middle by a very ſlender prop, was expoſed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hotteſt part, and the end next the muzzle projecting beyond the furnace, and ſurrounded with cloths which were drenched with water from time to time. (This arrangement is ſhewn in fig. 7.) In this ſituation, the fuſible metal ſurrounding the cradle being melted, the air contained in the air-tube would of courſe ſeek the higheſt poſition, and its firſt place in the air-tube would be occupied by fuſible metal. (In fig. 6, the new poſition of the air is ſhewn at *p q*).

Method of diſengaging the contents after experiment.

At the concluſion of the experiment, the metal was generally removed by placing the barrel in the tranſverſe muffle, with its muzzle pointing a little downwards, and ſo that the heat was applied firſt to the muzzle, and then to the reſt of the barrel in ſucceſſion. (This operation is ſhewn in fig. 8.) In

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place at the 28th degree; but I am convinced [illegible] his obſervations muſt have been made with ſome ſet different from that which was afterwards ſold.

ſome of the firſt of theſe experiments, I looſened the cradle, by plunging the barrel into heated brine, or a ſtrong ſolution of muriate of lime; which laſt bears a temperature of 250° of Fahrenheit before it boils. For this purpoſe, I uſed a pan three inches in diameter, and three feet deep, having a flat baſon at top to receive the liquid when it boiled over. The method anſwered, but was troubleſome, and I laid it aſide. I have had occaſion, lately, however, to reſume it in ſome experiments in which it was of conſequence to open the barrel with the leaſt poſſible heat*.

By theſe methods I made a great number of experiments, with reſults that were highly intereſting in that ſtage of the buſineſs, though their importance is ſo much diminiſhed by the ſubſequent progreſs of the inveſtigation, that I think it proper to mention but very few of them.

Calcareous ſpar converted into hard denſe marble by heat of 33° Wedgwood.

On the 31ſt of March, 1801, I rammed forty grains of pounded chalk into a tube of green bottle-glaſs, and placed it in the cradle as above deſcribed. A pyrometer in the muffle along with the barrel indicated 33°. The barrel was expoſed to heat during ſeventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one ſolid maſs, which had viſibly ſhrunk in bulk, the ſpace thus left within the tube being accurately filled with metal, which plated the carbonate all over without penetrating it in the leaſt, ſo that the metal was eaſily removed. The weight was reduced from forty to thirty-ſix grains. The ſubſtance was very hard, and reſiſted the knife better than any reſult of the kind previouſly obtained; its fracture was cryſtalline, bearing a reſemblance to white ſaline marble; and its thin edges had a decided ſemitranſparency, a circumſtance firſt obſerved in this reſult.

Calcareous ſpar rendered cryſtalline with rhomboidal fracture by heat 23°.

On the 3d of March of the ſame year, I made a ſimilar experiment, in which a pyrometer-piece was placed within the barrel, and another in the muffle; they agreed in indicating 23°. The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have loſt 3½ grains. A thin rim, leſs than the 20th of an inch in thickneſs, of whitiſh matter, appeared on the outſide of the maſs. In other reſpects, the carbonate was in a very perfect ſtate; it was of a yellowiſh colour, and had a decided ſemitranſparency and ſaline fracture. But what renders this reſult of the greateſt value, is, that on breaking the maſs, a ſpace of more than the tenth of an inch ſquare, was found to be completely cryſtallized, having acquired the rhomboidal fracture of calcareous ſpar. It was white and opaque, and preſented to the view three ſets of parallel plates which are ſeen under three different angles. This ſubſtance, owing to partial calcination and ſubſequent abſorption of moiſture, had loſt all appearance of its remarkable properties in ſome weeks after its production; but this appearance has ſince been reſtored, by a freſh fracture, and the ſpecimen is now well preſerved by being hermetically incloſed,

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* In many of the following experiments, lead was uſed in place of the fuſible metal, and often with ſucceſs; but I loſt many good reſults in this way: for the heat required to liquefy the lead approaches ſo near to redneſs, that it is difficult to diſengage the cradle without applying a temperature by which the carbonate is injured. I have [illegible] it anſwer well, to ſurround the cradle and a few inches of the [illegible] with fuſible metal, and to fill the reſt of the barrel with lead.

(To be continued.)

XIII.

On the Uſe of the Sutures in the Skulls of Animals. By Mr. B. Gibson *.

Conjectures on the uſe of the ſutures in the ſkulls of animals.

THE full uſe of the ſingular junction of the bones of the ſkull, which is called ſuture, has, from the earlieſt periods of anatomy and ſurgery, attracted the attention and eluded the reſearches of the phyſiologiſt. To this remarkable feature in oſteogny, in a great meaſure peculiar to a certain period of life, many uſes have been attributed. Some of theſe are totally erroneous; ſuch as that for allowing the tranſpiration of moiſture, to keep the brain cool and fit for thinking; for giving a more ſtrict adheſion of the *dura mater* to the inner ſurface of the ſkull; for admitting a more free communication by blood-veſſels between the external and internal parts of the head; or for affording interſtices, that the bones may be

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Mancheſter Memoirs, N. S. Vol. I. 39.

puſhed aſunder by the growth of the brain, leſt that organ ſhould be cramped in its growth, in conſequence of the comparatively ſlow growth of the bones of the ſkull.

Other ſuppoſed uſes.

Other uſes attributed to the ſutures are merely ſlight advantages derived from their ſtructure, which are enjoyed in early infancy, or till adult life, but gradually ceaſe after that period. Thus at the time of birth the looſe union of the bones of the ſkull accommodates the ſhape of the head to the figure of the different parts of the cavity through which it paſſes. At adult age, when the ſutures are fully formed, they may occaſionally check the progreſs (if I may be allowed the expreſſion) of a fracture nearly ſpent;—or vibrations, communicated to the bones of the ſkull, will be propagated with leſs force to the brain, in conſequence of the bones being ſeparated at the ſutures. It is, however, abundantly evident, that theſe are not the main purpoſes for which the ſutures are formed; otherwiſe they would not begin to be obliterated at a period of life when they would perform theſe offices more uſefully than ever. Conſiſtent with this remark we ſhall find, that the true purpoſe for which they are formed, and the particular proceſs with which they are connected, is fully completed before their obliteration takes place.

The cartilage between bones deſtined to be united, diſappears at laſt.

When we take a view of the mode of junction between many bones, and parts of bones in the human body, which do not admit of motion, we find that with little exception they all agree in this particular; that ſooner or later the cartilage or perioſteum which once was interpoſed is obliterated, and theſe different portions, or entire bones, coaleſce.

Inſtances [illegible] the [illegible] and [illegible] bones.

The ſeparate portions, which originally compoſe the vertebræ, are early in thus uniting: after theſe the ſides of the lower jaw; at a later period the epiphyſis of a cylindrical bone is united to its body: and ſtill later the bones of the ſkull uſually coaleſce, and the ſutures are obliterated. Other bones, as thoſe of the face, which have no motion and ſuſtain little weight, are irregular in this reſpect; ſometimes uniting, but generally remaining diſtinct, to the end of a long life.

Manner in which the oſſeous ſyſtem is completed, &c.

The original formation of the oſſeous ſyſtem in ſeveral diſtinct pieces, reſpects principally its ſpeedy oſſification at an early period of life, and its future convenient extenſion, till it has arrived at its full growth; and we may conſider it as a general a general principle, that where two parts of *one* bone are ſeparated from each other by an intervening cartilage, or *two* diſtinct bones merely by perioſteum, at that part oſſeous materials are added to increaſe their length or extend their ſuperficies. This we ſhall find takes place, whether the junction be effected by comparatively ſmooth ſurfaces, as between the body of a bone and its epiphyſis; or, between the bones of the ſkull by jagged ſutures. Hence it appears that the bones of the body generally are increaſed in length or extent, not by a uniform extenſion of the whole ſubſtance, but by an addition of bony matter in ſome particular part.

Cylindrical bones are lengthened by additions at each end.

Thus the body of a cylindrical bone is lengthened by addition to each end. This we might conclude would be the caſe, from conſidering the part in which its oſſification commences: as this commences in a middle point and proceeds to each extremity, it is natural to ſuppoſe that its growth ſtill goes on in the ſame direction, or continues at the extremities. That this is the caſe we know, not by reaſoning alone, but by a direct experiment. Mr. Hunter ſunk two ſmall pieces of lead in the middle of the tibia, or ſhin bone of a pig, and meaſured accurately the diſtance between them; on examining the animal ſome time afterwards, it appeared, that though the bone had increaſed conſiderably in length, the pieces of lead ſtill remained at the ſame diſtance from each other that they were before. From this experiment we learn, that a cylindrical bone is not extended in its middle, but is lengthened by addition to its extremities, where the body of the bone is joined to its epiphyſis; the chief intention of the epiphyſis being to allow the intervention of a vaſcular organ, which may conveniently depoſit bony materials, without interfering with the joint itſelf.

The ſame proceſs appears to take place at the edges of flat bones.

As cylindrical bones are lengthened at their extreme parts, we are led by analogy to conclude, that the ſame general plan is purſued in the extenſion of the flat bones of the body: and although we have no direct experiment by which this has been proved, there are circumſtances which leave little doubt but they are extended by addition to their edges. Thus to take the parietal bone as an example; as oſſification begins in a central point and extends towards the circumference, it is probable that to the completion of the proceſs, it continues to go on in the ſame direction; and the ſame circumſtance taking place in every bone of the cranium, it is probable that even after the whole of the brain is incaſed in bone, the addition is ſtill made at the edge of each, and that the general enlargement originates where they are all mutually joined by the ſutures. Of this proceſs I had a very ſtriking illuſtration ſome years ago. In a young ſubject, from what cauſe I know not, the depoſition of oſſeous matter had been ſuddenly increaſed a ſhort time before death. It was in different ſtages of progreſs, but had taken place in all the bones of the body which I preſerved; in ſome partially, in others generally. In all, the new oſſeous matter was elevated above the level of the bone upon which it was placed. In ſome parts of the parietal bones it was only in its commencement, and put on the appearance of a net-work, ſimilar to that which may be obſerved in the ſame bones at an early period of their formation. In other parts the meſhes of the net-work were more or leſs filled up; in others again completely, ſo as to put on the uniform appearance of ſolid bone. The ſame reticulated appearance was evident on the edges of all the bones of the ſkull, where they form the ſutures, and at the extremities of the cylindrical bones, between the body and epiphyſis. The ſame appearance of increaſed depoſition was ſeen on the ſurface of the cylindrical bones, with this difference, that the meſhes were not circular, but oblong ſquares; ſo as to put on more of the ſtriated appearance. In ſome parts, the newly ſecreted bone was eaſily ſeparable from the general maſs, and formed a thin layer externally, affording one of the beſt proofs I have met with, of the increaſe of cylindrical bones in thickneſs by depoſition externally, whilſt a correſponding internal abſorption goes on. From the ſtriking ſimilarity of appearance on the ſurfaces and edges of the bones, we may ſafely conclude, that the ſame proceſs of depoſition was going on in both, and may thence infer, that the bones of the ſkull are increaſed in extent by the depoſition of oſſeous matter at their edges, or where they are joined to each other by ſuture. [illegible] fact points out to us, in a great meaſure, the real uſe of this peculiar mode of junction.

Inſtance in the ſkull.

In order that the bones of the ſkull may be increaſed in extent, it is neceſſary that they ſhould be retained at a certain diſtance from each other; that the perioſteum with its veſſels

The ſerrated edges give firmneſs, &c.

may paſs down between them, free from compreſſion and ſecrete the oſſeous matter. At the ſame time, the thin bones compoſing the upper part of the ſkull, reſting as an arch upon its baſis, muſt be united together ſo firmly, as not to be ſeparated by common degrees of violence. For this purpoſe, projecting points from the external ſurface of each bone, are reciprocally received into correſponding nitches; which only penetrate through one half of the thickneſs of the ſkull, and form an irregular kind of dovetailing.

Two advantages ariſe from this ſtructure, being ſuperficial, and confined to the external table of the ſkull. The projecting points from each ſide, reſting upon the ſolid ſurface of the internal table of the oppoſite bone, can reſiſt more effectually any violence, which might tend to force the bones inwards; and the internal part of the ſkull preſents, by this means, a ſmooth ſurface to the coverings of the brain; for internally no appearance of a jagged ſuture is ſeen.

Thus the ſutures unite the bones, and admit the vaſcular organ requiſite for their growth.

From this view of the ſubject we ſee, that the ſutures of the human ſkull, by their peculiar formation, at once unite the bones together, and ſo far ſeparate them, as to allow the interpoſition of a vaſcular organ by which their ſuperficies is gradually increaſed to its greateſt extent *. This explanation of

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* Since this paper was written in the year 1800, I have found, that a ſimilar opinion was publiſhed by Profeſſor Soemmerring in 1794, in his valuable work, " De corporis humani fabrica." To him, therefore, any credit which may belong to the primary ſuggeſtion of this uſe of the ſutures is due. As his opinion, however, has been little noticed by anatomiſts generally, and is placed in a clearer point of view by the facts which ſuggeſted this further explanation of it to me, it has not been thought improper to give this eſſay a place in theſe Memoirs. But whilſt the reader will ſee, by the following quotation, the near reſemblance between the opinion of Profeſſor Soemmerring and that which I have brought forward, I hope the character of plagiariſt or compiler will not be attributed to me.

" Uſu horum ſic ſeſe habentium terminorum oſſa cranii inter bene liquet.

" Incrementum ambitus calvariæ levant, ni enim inter oſſa capitis mox poſt partum ſuturæ interponerentur, hæc creſcere non poſſent, niſi aliâ ratione natura rem inſtitueret. Tali igitur modo incrementum calvariæ cum incremento reliquorum oſſium convenit; initio enim ſuturis, vel potius lineis cartilaginoſis oſſa iis

of the uſe of ſutures comprehends and accounts for thoſe concomitant circumſtances, which were conſidered by older anatomiſts as their real uſe; and, as far as I can ſee, is not contradicted by any fact connected with them.

Other remark and inferences

If it be aſked, for inſtance, why at the ſutures there is a ſtronger adheſion of the *dura mater* internally and *perioſteum* externally than in other parts of the ſkull? the anſwer is, that theſe membranes with their veſſels are continued into the ſutures, to form conjointly the ſecretory organ, by which the bones are extended.

If it be aſked, why there is a greater vaſcularity or an appearance of blood-veſſels paſſing through the ſutures? it is perfectly conſiſtent with this opinion to anſwer, that the increaſe of blood goes to this ſecretory organ, for the purpoſe of the extenſion of the bones.

Why the ſutures are obliterated, &c.

The explanation here offered accounts alſo for the general obliteration of the ſutures after a certain period of life; for the bones having then arrived at their full ſize, the organ for the ſecretion of oſſeous matter is no longer needed; it ſhrinks and is abſorbed, and the bones gradually coaleſce; by which a further advantage is derived, that of an acceſſion of ſtrength to the cranium at large.

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iis locis conglutinantur, verum tamen non niſi in embrionibus ad fonticulos, ut aiunt, hæc linea notabili latitudine, obſervatur. Oſſibus enim capitis hic locorum cerebro creſcente, placide quaſi deductus, cartilago augetur, latior evaſura, niſi priſtina pars ſimul in os mutaretur, inde oſſa calvariæ, eodem modo, quo oſſa longa deductis epiphyſibus, vel quod unum idemque eſt, marginibus creſcere, liquet, etſi in oſſibus, longis ſutura epiphyſes inter et diaphyſin non criſpetur.

"Quo junior igitur infans, eo minus criſpa et implexa ſutura, vel ut rectius loquar, linea cartilaginoſa auguſta, oſſa jungens, obſervatur. Quum vero aucta ætate oſſa, creſente cerebro, deducuntur, eorumque, craſſitudo adpoſita cum internæ, tum externæ potiſſimum tabulæ, (internæ enim incrementum citius [illegible] videtur) maſſa oſſea, augetur, non poteſt non eſſe, quin hæc criſpa ſuturæ forma, quum quidem naſci cœpit, externâ in ſuperficie tamdiu, augeatur, donec tandem ipſa ea quam maxime impediat, quo minus cerebrum calvariam ulterius deducere poſſit, quod pubertatis tempore accidit. Rariſſime hæc oſſificatio ad ætatem virilem uſque detinetur."—Soemmerring de corporis Humani Fabrica, page 912.

If any additional argument be neceſſary in ſupport of this opinion, I may alſo notice the ſtriking analogy which ſubſiſts between the ſeparation of one bone of the ſkull from another by a ſuture; and that ſeparation which exiſts between the body of a cylindrical bone and its epiphyſis. They each remain only for a certain length of time; each allows the interpoſition of a ſecretory organ; and both begin to be obliterated when the bones with which they are connected have completed their growth, and their continuance is no longer neceſſary.

XIV.

On the Reproduction of Buds. By THOMAS ANDREW KNIGHT, *Eſq. F. R. S.**

MY DEAR SIR,

If the generated buds of a ſeaſon be deſtroyed, others are produced.

EVERY tree in the ordinary courſe of its growth generates, in each ſeaſon, thoſe buds which expand in the ſucceeding ſpring; and the buds thus generated, contain, in many inſtances, the whole of the leaves which appear in the following ſummer. But if theſe buds be deſtroyed during the winter or early part of the ſpring, other buds, in many ſpecies of trees, are generated, which in every reſpect perform the office of thoſe which previouſly exiſted, except that they never afford fruit or bloſſoms. This reproduction of buds has not eſcaped the notice of naturaliſts; but it does not appear to have been aſcertained by them from which, amongſt the various ſubſtances of the tree, the buds derive their origin.

Du Hamel's opinion that theſe laſt are from pre-organized germs.

Du Hamel conceived that reproduced buds ſprang from pre-organized germs; but the exiſtence of ſuch germs has not, in any inſtance, been proved, and it is well known that the roots, and trunk, and branches of many ſpecies of trees will, under proper management, afford buds from every part of their ſurfaces; and therefore, if this hypotheſis be well founded, many millions of ſuch germs muſt be annually generated in every large tree; not one of which in the ordinary courſe of nature will come into action: and as nature, amidſt all its exuberance

Objection.

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* Phil. Tranſ. 1805.

berance, does not abound in uſeleſs productions, the opinions of this illuſtrious phyſiologiſt are, in this caſe, probably erroneous.

Suppoſition that they are afforded by the bark.

Other naturaliſts have ſuppoſed the buds, when reproduced, to ſpring from the plexus of veſſels which conſtitutes the internal bark; and this opinion is, I believe, much entertained by modern botaniſts: it nevertheleſs appears to be unfounded, as the facts I ſhall proceed to ſtate will evince.

Inſtance to the contrary in ſea cale. Internal buds.

If the fruit-ſtalks of the ſea cale (*crambe maritima*) be cut off near the ground in the ſpring, the medullary ſubſtance, within that part of the ſtalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the ſucceeding winter. The ſides of this cup conſiſt of a woody ſubſtance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum, as the white wood of the oak or elm: and from the interior part of this ſubſtance, within the cup, I have frequently obſerved new buds to be generated in the enſuing ſpring. It is ſufficiently obvious that the buds in this caſe do not ſpring from the bark; but it is not equally evident that they might not have ſprung from ſome remains of the medulla.

Potatoes afford buds at the cut ſurface,

In the autumn of 1802, I diſcovered that the potatoe poſſeſſed a ſimilar power of reproducing its buds. Some plants of this ſpecies had been ſet, rather late in the preceding ſpring, in very dry ground, where, through want of moiſture, they vegetated very feebly; and the portions of the old roots remained ſound and entire till the ſucceeding autumn. Being then moiſtened by rain, many ſmall tubers were generated on the ſurfaces made by the knife in dividing the roots into cuttings; and the buds of theſe, in many inſtances, elongated into runners, which gave exiſtence to other tubers, ſome of which I had the pleaſure to ſend to you.

—and therefore not from the bark.

I have in a former paper remarked, that the potatoe co[illegible] of four diſtinct ſubſtances, the epidermis, the true ſkin, the bark, and its internal ſubſtance, which, from its mode of formation, and ſubſequent office, I have ſuppoſed to be alburnous: there is alſo in the young tubes a tranſparent line through the centre, which is probably its medulla. The buds and runners ſprang from the ſubſtance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the ſurface in contact with the bark. It muſt, however, be admitted, that the internal ſubſtance of the potatoe correſponds more nearly with our ideas of a medullary than of an alburnous ſubſtance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of theſe plants are not generated by the cortical ſubſtance of the root: and I ſhall proceed to relate ſome experiments on the apple, and pear, and plumb-tree, which I conceive to prove that the reproduced buds of thoſe plants do not ſpring from the medulla.

Other inſtances of fruit trees in which reproduced buds appeared to ſpring from the alburnum.

Having raiſed from ſeeds a very conſiderable number of plants of each of theſe ſpecies in 1802, I partly diſengaged them from the ſoil in the autumn, by digging round each plant, which was then raiſed about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the ſeed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained expoſed to the air and light. In the beginning of April, I obſerved many ſmall elevated points on the bark of theſe roots, and, removing the whole of the cortical ſubſtance, I found that the elevations were occaſioned by ſmall protuberances on the ſurface of the alburnum. As the ſpring advanced, many minute red points appeared to perforate the bark: theſe ſoon aſſumed the character of buds, and produced ſhoots, in every reſpect ſimilar to thoſe which would have ſprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I ſhall not venture to decide; but I am much diſpoſed to believe that, like thoſe of the potatoe, they ſprang from the alburnous ſubſtance ſolely.

They do not originate from the medulla.

The ſpace, however, in the annual root, between the medulla and the bark is very ſmall; and therefore it may be contended that the buds in theſe inſtances may have originated from the medulla. I therefore thought it neceſſary to repeat ſimilar experiments on the roots and trunks of old trees, and by theſe the buds were reproduced preciſely in the ſame manner as the annual roots: and therefore, conceiving myſelf to have proved in a former Memoir,* that the ſubſtance which has been called the medullary proceſs does not originate from the medulla, I muſt conclude that reproduced buds do not ſpring from that ſubſtance.

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* Phil. Tranſ. of 1803.

Remarks on the manner in which this proceſs of nature is probably effected.

I have remarked, in a paper which you did me the honour to lay before the Royal Society in the commencement of the preſent year, that the alburnous tubes at their termination upwards invariably join the central veſſels, and that theſe veſſels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give exiſtence to new buds and leaves. It is alſo evident, from the facility with which the riſing ſap is transferred from one ſide of a wounded tree to the other, that the alburnous tubes poſſeſs lateral as well as terminal orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may poſſeſs the power to generate central veſſels; which veſſels evidently feed, if they do not give exiſtence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither ſpring from the medulla nor the bark, I am much inclined to believe that they are generated by central veſſels which ſpring from the lateral orifices of the alburnous tubes. The practicability of propagating ſome plants from their leaves may ſeem to ſtand in oppoſition to this hypotheſis; but the central veſſel is always a component part of the leaf, and from it the bud and young plant probably originate.

Attempt to diſcover the ſame power in ſeeds.

I expected to diſcover in ſeeds a ſimilar power to regenerate their buds; for the cotyledons of theſe, though diſſimilar in organization, execute the office of the alburnum, and contain a ſimilar reſervoir of nutriment, and at once ſupply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been deciſive, owing to the difficulty of aſcertaining the number of buds previouſly exiſting within the ſeed. Few, if any, ſeeds, I have reaſon to believe, contain leſs than three buds, one only of which, except in caſes of accident, germinates; and ſome ſeeds appear to contain a much greater number. The ſeed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the ſeeds, like the buds of the horſe-cheſtnut, contain all the leaves and apparently all the buds of the ſucceeding year: and I have never been able to ſatisfy myſelf that all the buds were eradicated without having deſtroyed the baſe of the plumule, in which the power of reproducing buds probably reſides, if ſuch power exiſts.

Annual and biennial plants have not this power.

Nature appears to have denied to annual and biennial plants (at leaſt to thoſe which have been the ſubjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheleſs ſome biennials poſſeſs, under peculiar circumſtances, a very ſingular reſource, when all their buds have been deſtroyed. A turnip, bred between the Engliſh and Swediſh variety, from which I had cut off the greater part of its fruit-ſtalks, and of which all the buds had been deſtroyed, remained ſome weeks in an apparently dormant ſtate; after which the firſt ſeed in each pod germinated, and burſting the ſeed-veſſel, ſeemed to execute the office of a bud and leaves to the parent plant, during the ſhort remaining term of its exiſtence, when its preternatual foliage periſhed with it. Whether this property be poſſeſſed by other biennial plants in common with the turnip, or not, I am not at preſent in poſſeſſion of facts to decide, not having made preciſely the ſame experiment on any other plant.

Correction of a former inference.

I will take this opportunity to correct an inference that I have drawn in a former paper,* which the facts (though quite correctly ſtated) do not, on ſubſequent repetition of the experiment, appear to juſtify. I have ſtated, that when a perpendicular ſhoot of the vine was inverted to a depending poſition, and a portion of its bark between two circular inciſions round the ſtem removed, much more new wood was generated on the lower lip of the wound become uppermoſt by the inverted poſition of the branch, than on the oppoſite lip, which would not have happened had the branch continued to grow erect; and I have inferred that this effect was produced by ſap which had deſcended by gravitation from the leaves above. But the branch was, as I have there ſtated, employed as a layer, and the matter which would have accumulated on the oppoſite lip of the wound had been employed in the formation of roots, a circumſtance which at that time eſcaped my attention. The effects of gravitation on the motion of the deſcending ſap, and conſequent growth of plants, are, I am well ſatisfied, from a great variety of experiments, very great; but it will be very difficult to diſcover any method by which the extent of its operation can be accurately aſcertained. For the veſſels which convey and impel[*] the true ſap, or fluid from which the new wood appears to be generated, paſs immediately from the leaf-ſtalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unleſs it had been extravaſated, could have deſcended to the part from which the bark was taken off in the experiment I have deſcribed. I am not ſenſible that in the different papers which I have had the honour to addreſs to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of ſupporting.

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* Phil. Tranſ. of 1803.

I am, &c.

THOˢ. ANDREW KNIGHT.

Elton, May 12, 1805.

XV.

Experiments on the Gaſeous Oxide of Azote, by a Society of Amateurs at Toulouſe. Publiſhed by M. P. DISPAN, *Profeſſor of Chemiſtry in the College of that City.*[*]

Diſagreement of former experiments on the oxide of azote.

THE motive for the following experiments was the very different, and even contradictory reſults, which have been publiſhed of former effects. The experiments were tried upon more than a dozen perſons, and in ſome caſes repeated two or three times; the ſenſations which each experienced were written down at the moment, by the reporter, from whoſe memorandums the ſubſequent obſervations are drawn.

Preparation of the nitrate of ammonia.

The nitrate of ammonia uſed for the experiment was indiſtinctly cryſtallized, but was quite neutral. Its taſte was very pungent, with a ſlight odour. It had been formed by ſaturating very pure nitric acid with ammoniacal gas obtained by diſtilling ſal ammoniac with the common potaſh of commerce.

Proceſs for obtaining the gaſeous oxide of

About one hectogramme (1545 grains) of this ſalt was put into a ſmall retort, and placed on a ſand-bath, where the ſalt melted and boiled for a ſhort time without yielding any gas; at length, the retort became filled with a white vapour, which quickly diſappeared; the gas was then rapidly diſengaged, and was caught in bladders. By degrees the diſengagement became more and more ſlow, and when the operation was ended, ſcarcely any thing remained in the retort.

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[*] Phil. Tranſ. of 1804.

[†] Annales de Chimie, Vol. LVI. p. 243.

The ſame proceſs on a larger ſcale.

Another experiment was made with a larger retort, and three hectogrammes (10 oz. troy) of the ſalt, from which was obtained gas ſufficient to fill eight bladders. This operation proceeded in a ſimilar manner with the former; except that as the retort cooled, a red vapour aroſe within it, which it was aſcertained by experiment, contained no nitrous gas.

Effects of Gaſeous Oxide of Azote when breathed into the Lungs.

The gas has a ſaccharine taſte.

All who have taſted or inhaled this gas, agree in deſcribing its flavour as ſtrongly ſaccharine, and remaining upon the organs of ſome perſons during the whole day after receiving it. M. Diſpan obſerved in it an after-taſte of nitre; but acknowledges that it was the laſt collected gas which he taſted.—M. de M***, perhaps under a ſimilar impreſſion, ſays he perceived in it a ſtyptic quality.

The gas was inſpired.

The method of reſpiring this gas was by means of a bladder with a ſtop-cock in it, applied to the mouth; the noſtrils being cloſed, and the lungs as much as poſſible emptied.

No. 1. The firſt perſon upon whom the experiment was tried, ſwooned at the third inſpiration, and remained ſenſeleſs about five minutes, when he recovered, but with a ſenſation of great fatigue. He recollected to have experienced only a ſudden faintneſs, attended with a tingling at the temples.

No. 2. M. de M*** obſerved a ſaccharine and ſtyptic taſte, and experienced a ſenſe of great dilatation, accompanied with heat in the breaſt; his veins ſwelled, and his pulſe was quickened: ſurrounding objects ſeemed to revolve round him. But he thought he could have borne a ſtronger doſe; the bladder not being large enough for his lungs.

No. 3 experienced a ſaccharine taſte on the firſt inſpiration; but became inſenſible to thoſe which ſucceeded. His lungs were forcibly dilated with great heat. When the bladder was removed, he appeared very comfortable, but could not refrain from violent burſts of involuntary laughter.

No. 4 had the ſame ſaccharine taſte with the preceding, and retained the impreſſion from ten o'clock in the morning till after midnight. He experienced vertigoes, and his legs trembled under him during the remainder of the day.

No. 5, the ſame ſaccharine taſte. On quitting the bladder, he had a dizzineſs of ſight, which was ſucceeded by a ſenſation of great pleaſure throughout the body. His legs were weakened.

No. 6, Saccharine flavour throughout the day; tingling in the ears; legs tottering, and the ſtomach oppreſſed. All that he experienced was rather painful than agreeable.

Receiving the gas from a bladder, had no influence on the reſult of the experiment.

In order to aſcertain what influence the mode of breathing from a bladder might have on the foregoing reſults, the parties were requeſted to inſpire common air in the ſame manner.—They were all mechanically fatigued by it, and nothing more.

Oxigen gas differed from common air only by a ſmall increaſe of the heat of the lungs.

The bladders were next filled with oxygen gas, and applied as before to the ſame perſons, who found only a ſlight difference between it and common air, conſiſting in an augmentation of the heat of the lungs.

Concluſion.

The ſingular effects above deſcribed, can, therefore, and ought only to be aſcribed to the gaſeous oxide of azote.

Other experiments.

Another meeting of the ſociety was held for repeating the experiments more at large, on the reſpiration of gaſeous oxide of azote.

Deſcription of the apparatus with upwards of 2lbs. of the ſalt.

Eight hectogrammes (27½ oz. troy) of nitrate of ammonia, prepared as before, were put into a retort, with its neck fitted to a double-bodied receiver, from whence, by means of a tube of welter, the gas paſſed into an inverted veſſel over water. The retort was placed on a ſand-bath.

Particulars of the proceſs.

As ſoon as the heat affected the retort, the ſalt melted; and nearly at the ſame moment, ſparkling vapours aroſe in the retort, but in very ſmall quantity. The air which the heat expelled from the veſſels had a nitrous odour; but this as well as the vapours gradually diminiſhed, and as the proceſs continued they diſappeared altogether; they were ſucceeded by a lively ſmell of pruſſic acid. At length the retort became filled with white vapours, and the gaſeous oxide of azote began to paſs over. The diſengagement ſoon became ſo abundant that it was judged proper to draw out the fire; but afterwards, on replacing the coals, the gas, which in the interval had diminiſhed,

niſhed, was again ſo rapidly developed that the luting of the veſſels began to give way. But notwithſtanding the loſs which this occaſioned, the diſengagement continued extremely rapid in the receiver for at leaſt a quarter of an hour.

M. Diſpan ſuppoſes, that if the luting had not given way, an exploſion would have taken place, as has happened to others in this proceſs.

Danger of exploſion.

He next proceeds to ſtate the effects of the reſpiration of this gas.

Twelve perſons underwent the experiment, and on many was repeated. He obſerves that moſt of them had inhaled the gas of the former operation, where two out of ſeven experienced pleaſing ſenſations; but on this ſecond occaſion, not one felt pleaſure; on the contrary, they all felt pain, and many ſuffered extremely.

The effects produced by the laſt gas were more powerful than the former.

One perſon ſtamped with his foot the whole time of the breathing: when the bladder was removed, he recovered from the profound ſtupor into which he had been plunged, and complained of a pain in the back part of his head, as if he had received a violent blow from a dagger: he could not be prevailed on to make another trial. The other perſons in general were affected with vertigoes and dizzineſs of ſight, ſucceeded in ſome by involuntary convulſive fits of laughter.

M. Diſpan tried the effects of this gas on himſelf, which he thus deſcribes:—

M. Diſpan's deſcription of the effect of the gas upon himſelf.

"At the firſt inſpiration, I emptied the bladder, and my mouth was inſtantaneouſly filled with a ſaccharine flavour, which extended into my lungs and inflated them. I emptied and filled them again; but on the third attempt, my ears were filled with a tingling noiſe, and I dropped the bladder. I did not, however, become altogether inſenſible, but remained in a kind of benumbed aſtoniſhment, rolling my eyes about without fixing them on any particular object: I was then ſuddenly ſeized with convulſive laughing fits, ſuch as I never in my life before experienced. In a few ſeconds this propenſity to laugh ſtopped ſuddenly, and I no longer felt any unpleaſant ſymptom."

Two others on whom the gas was tried, experienced only a convulſive movement of ſome of the muſcles of the face; but were in the courſe of the day attacked with violent diarrhœa.

Effects on two other perſon.

Difficulty of reducing the effects of this gas to any general ſyſtem.

M. Diſpan thinks it will be very difficult to reduce the effects of gaſeous oxide of azote to any general ſyſtem, as they vary ſo conſiderably in their operations upon different individuals, and, what is more ſingular, even upon the ſame perſon.

M. D. concludes his paper with an account of an experiment to aſcertain the effect of gaſeous oxide of azote upon animals.

Experiments on a bird immerſed in gaſeous oxide of azote.

He placed a greenfinch in a veſſel of ſufficient dimenſions, and filled it with gaſeous oxide of azote. At firſt, the bird ſeemed to ſuffer no inconvenience; but he ſoon gradually cloſed his eyes, and dropped gently on his ſide, as if aſleep. On being reſtored to the pure air, he reſumed his feet, without attempting to fly away. About an hour afterwards he was ſubjected to a ſecond trial, and having been ſuffered to remain longer in the veſſel, he was taken out quite dead.

M. Diſpan thinks it very remarkable that the bird ſhould make no effort to eſcape, and that he ſhould manifeſt no convulſive ſymptoms, ſuch as take place in experiments with other gaſes.

XVI.

Obſervations on the Mammoth, or American Elephant, by which it is proved to have been an herbivorous Animal. In a Letter from the Right Reverend Biſhop MADISON.*

Diſcovery of a mammoth having vegetable remains in its ſtomach.

ONE of thoſe facts has lately occurred, which the naturaliſt knows beſt how to appreciate, and which I therefore take a pleaſure in communicating to you. It is now no longer a queſtion, whether the Mammoth was a herbivorous or carnivorous animal. Human induſtry has revealed a ſecret, which the boſom of the earth had, in vain, attempted to conceal.—In digging a well, near a Salt-Lick, in Wythe-county, Virginia, after penetrating about five feet and a half from the ſurface, the labourers ſtruck upon the ſtomach of a mammoth. The contents were in a ſtate of perfect preſervation, conſiſting of half maſticated reeds, twigs, and graſs, or leaves. There could be no deception; the ſubſtances were deſignated by obvious characters which could not be miſtaken, and of which every one could judge; beſides, the bones of the animal lay around, and added a ſilent, but ſure confirmation. The whole reſted upon a lime-ſtone rock. I have not ſeen, as yet, any part of thoſe contents; for, though I was within two days' journey of the place where they were found, I was ſo well ſatisfied with the narration of gentlemen who had ſeen them, and upon whoſe veracity, as well as accuracy, I could rely, that I thought the journey unneceſſary; eſpecially as I took meaſures to enſure the tranſmiſſion of a ſufficient quantity of the contents, together with all the bones, to Williamſburgh. When the contents arrive, a part ſhall be forwarded to you. I hope to form a complete ſkeleton of this vaſt animal, having given directions to ſpare no labour, in digging up every bone.

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* To Benjamin Smith Barton, M. D. editor of the Philadelphia Medical and Phyſical Journal, from which (vol. II.) it is taken.

Remarks on the preſervation of thoſe bodies,

We ſhould not be ſurpriſed, that theſe ſubſtances ſhould be thus preſerved, when we recollect the ſtate of the rhinoceros, mentioned by Pallas. Blumenbach, in his *Manuel d'Hiſtoire Naturelle*, vol. II. p. 398, (traduit par Artaud), has a note, which is very applicable to the preſent ſubject. He ſays, "Quelquefois on trouve encore des pièces animales qui ont conſervé, ſans alteration, leurs parties *molles;* mais, cependant, comme elles ſe trouvent auſſi enfouies dans la terre par la ſuite de ces grandes cataſtrophes des temps antérieurs, on doit les ranger parmi les corps pétrifiés, dans le ſens le plus étendu. Je citerai, par exemple, le rhinocéros déterré près de Wiloi, en Sibérie, qui offroit encore des reſtes tres-reconnoiſſables, même ayant encore l'odeur animal de muſcles, de chair, de peau, & de poils. Pallas l'a décrit tres-exactement dans les Nov. Comment. Petropolit., tome 13. p. 585."

—probably from marine ſalt.

Whether this firſt kind of petrifaction, of which Blumenbach ſpeaks, and which he calls *ſimplement calcinés*, has been the cauſe of the preſervation of theſe ſubſtances, or whether it be the effect of the marine ſalt, with which the earth, where they were buried, has been conſtantly charged, muſt be left to future inveſtigation. I pretend not to decide. Had they been buried deep in the earth, that circumſtance alone might have prevented a decompoſition; but the depth of five or ſix feet ſeems inſufficient to arreſt that chemical action, which changes the appearances of organized bodies. The fact, however, is deciſive, as to the principal queſtion. It has ſummoned the diſcordant opinions of philoſophers before a tribunal, from which there is no appeal.

Williamſburgh, October 6th, 1805.

Note on the preceding Paper. By the Editor.

Facts by Mr. Nevil on long preſerved vegetable bodies.

Mr. Francis Nevil, in his account of the elephantine teeth that were diſcovered in the north of Ireland, early in the eighteenth century, has mentioned ſome facts relative to the long preſervation of vegetable matters, which ſeem worthy of our notice in this place: and the more ſo, as this gentleman's paper ſeems not to have excited any attention among the modern writers on the exuviæ of animals found in countries in which the living animals themſelves are no longer ſeen. Some extravagant conjectures are mixed with Mr. Nevil's account: but theſe do not, in the leaſt, invalidate the truth of what he ſays, relative to the bed upon which the Iriſh elephant was laid.

His narrative.

"The place (ſays he) where this monſter lay, was thus prepared, which makes me believe it had been buried, or that it had lain there ſince the deluge. It was about four feet under ground, with a little riſing above the ſuperficies of the earth, which was a plain under the foot of a hill, and about thirty yards from the brook* or thereabout. The bed whereon it lay had been laid with fern, with that ſort of ruſhes here called ſprits, and with buſhes intermixed. Under this was a ſtiff blue clay on which the teeth and bones were found: above this was firſt a mixture of yellow clay and ſand much of the ſame colour; under that a fine white ſandy clay, which was next to the bed: the bed was for the moſt part a foot thick, and in ſome places thicker, with a moiſture clear through it; it lay ſad and cloſe, and cut much like turf, and would divide into flakes, thicker or thinner as you would; and in every layer the ſeed of the ruſhes was as freſh as if new pulled, ſo that it was in the height of ſeed-time that thoſe bones were laid there. The branches of the fern, in every

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* "A ſmall brook that parts the counties of Cavan and Monaghan."

lay

ly as we opened them, were very diſtinguiſhable, as were the ſeeds of the ruſhes and the tops of the boughs. The whole matter ſmelt very ſour as it was dug, and tracing it I found it 34 feet long and about 20 or 22 feet broad."—" I forgot to mention that there was a great many nut-ſhells found about the bed, perhaps thoſe might have been on the buſhes which compoſed part of the bed *."

XVII.

Obſervations on the Danger of uſing Earthen-ware or Pottery of a bad Quality. By M. OIDEVIN *of Rouen* †.

Different kinds of pottery.

PURE white argil forms the body of the fineſt pottery which bears the name of porcelain; clays leſs pure, and coloured more or leſs with iron, ſerve to form the ſtone ware, or hard earthen-ware, and the common or ſoft ſort, which differs from the other, in not experiencing a commencement of fuſion at their ſurface in baking, like porcelain or ſtone ware.

This badly prepared common earthen-ware is the kind which is occaſionally attended with danger in its uſe, and is the ſubject of this paper.

Earthen-ware.

Common brown ware.

The biſcuit of brown earthen-ware is prepared from a ferruginous clay; that of white earthen-ware is compoſed of a mixture of ferruginous clay, of another clay containing much ſilicious ſand, a little lime, and finally of a porous clay, which renders it leſs compact, and gives it whiteneſs after baking.

Differences in the ware from the quality the materia

Nature not always affording theſe earths in the ſame ſtate of combination, occaſions differences in the biſcuit, when it becomes ſubjected to the heat: other differences alſo ariſe in the action of the enamel on the biſcuit. If the earth be too ferruginous, or too much mixed with ſilicious particles, the enamel, during the baking, acts as a flux on the biſcuit, ſoftens it, and occaſions the pieces to loſe their ſhape.

If the earth is too porous it abſorbs the enamel and remains rough, and as it were dried. If it contains too much lime, it throws off the enamel, which falls from it in ſcales inſtead of adhering to it.

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* A Natural Hiſtory of Ireland, in three parts, by Dr. Gerrard Boate, Thomas Molineux, M.D. F.R.S. and others. Pages 128—130. Dublin: 1755.

† Annales de Chemie, T. 55.

Compoſition of the enamel or glaze.

On the other hand, the white enamel is compoſed of ſilicious ſand, a little lime, lead and tin oxides, and ſome flux, ground together with water in mills. The brown ſort is compoſed of the ſame materials, with the addition of manganeſe and perigord ſtone*.

Cauſes which occaſion variations in the glaze.

The greater or leſs fuſibility of the ſand; the greater or leſs purity of the lead, of the tin, and of the ſaline ſubſtances employed as fluxes; the different degrees of heat which the mixture receives in the glazing; the variations of the fineneſs given to the glazing materials by the action of the mill, are ſo many circumſtances which cauſe changes in the enamel in its ſtate of fuſion on the pieces, relative to the ſtate in which it finds the biſcuit and to the fuſible layer, with which this laſt is covered.

Pottery.

Brown pottery.

The body of the brown pottery is a red clay, more or leſs ferruginous and compact according to the places where it is procured.

Yellow pottery.

The common or yellow pottery is made of a white clay, which contains a little lime and magneſia, and a conſiderable quantity of ſilicious ſand, which may be generally eſteemed a fourth of the maſs.

Glaze for brown ware.

The glazing of the brown pottery is formed with a mixture of ſilicious ſand, yellow or red oxide of lead, and manganeſe pulveriſed together.

Glaze for yellow ware.

Mottled ſtreaks in foreign ware.

That of the yellow earthen-ware is compoſed of a mixture of ſilicious ſand, and red oxide of lead, which, during its baking vitrifies at its ſurface, and forms a yellow glazing more or leſs tranſparent. To this mixture is commonly added, in France, a little oxide of magnaneſe in powder, more or leſs fine, without grinding them together. This is called the *grain*, becauſe it fuſes more difficultly than the other materials, without mixing with them, and by that means forms ſtreaks, ſpots, or brown ſpecks, according to the coarſeneſs of the powder itſelf.

Cloudy tinges in the glaze.

In ſome manufactories they mix oxide of copper with the common glazing, to give it a green colour, and in others they form deſigns on the pieces, with oxide of copper, which produces a green; with oxide of iron, which cauſes a red, or with oxide of manganeſe, which gives a brown.

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* A black ſtone or compact manganeſe. T.

Cauſes of imperfection in pottery.

Great imperfections are produced in pottery, from the injudicious uſe of glazing over earths of an unſuitable nature, and this is more remarkable when the earths are not ſo well prepared for their glazings as they are for thoſe of the finer wares. The articles of common pottery are leſs carefully prepared both in their materials and baking. This laſt is uſually performed at a ſingle operation, and with leſs fire.

Cautions requiſite to inſure its goodneſs.

The means of producing good pottery and earthen-ware, conſiſt in carefully chuſing the earths for forming the body: in producing an exact coincidence of expanſion by heat between them, and the vitrifiable glaze with which they are to be covered, and in baking them by a proper degree of fire, produced from combuſtibles not capable of changing the nature of the glazing.

The neglect of theſe attentions occaſion defects in the manufactured articles, which are either unſightly and nothing more, or both unſightly and dangerous.

Defects or deformities enumerated.

The unſightly defects which are found in ill conditioned pottery or earthen-ware, are, *ſcaling;* the *dropping* or *drops;* *ſmoke;* *drying* of the ware, and *flaws* or *cracks.*

Scaling of the glaze.

The ſcaling is the appellation uſed when the glazing of a piece detaches itſelf in ſcales, by the action of moiſt air, or on the leaſt touch, and leaves the biſcuit uncovered.

Dropping or drops.

The *dropping* or *drops* take place when the moiſture of the fuel having ſtruck the pieces during the baking, the enamel is collected in *drops* on the ſurface, and remains vitrified in that form, inſtead of being equally ſpread.

Smoky tinge.

The ſmoky appearance happens when a piece has not been purified by a clear flame, but remains blackened or ſtained.

Drying.

The drying happens when the pieces are, as it were, roaſted in the firing, and come out rough from the abſorption of the enamel into their ſubſtance.

Flaws.

The flaws happen, when the earth or the biſcuit, having a different pyrometrical expanſibility from that of the enamel; the body contracts in cooling more than the glaze which is therefore ſplit, or which is divided into an infinite number of ſmall parts, ſometimes not perceptible to the eye when the pieces are new, but which become very viſible, when the goods have imbibed any greaſy ſubſtance in uſing.

The coarſe p ery is moſt defective.

All theſe defects, though diſagreeable to the eye, have really, with regard to the ware itſelf, only the inconvenience of a dirty appearance, provided the biſcuit is always compact, and well baked. But it is different in the common pottery in which the dropping, the ſcaling, and the flaws produce more injurious defects. As the earth is more porous and leſs baked in thoſe, the liquids preſerved in them enter into the pores where they become altered and decompoſed, and produce ſulphurated hydrogen, which injures every thing kept in them.

Cavities or pits from bad firing.

The moſt noxious defects in pottery are the cavities or pits, and the underbaking. The pits are roughneſſes or hollow bubbles which are found on thoſe pieces, whoſe enamel being injured by rubbing, or being too little acted on by the fire, has not been fuſed into a vitreous ſubſtance. In theſe the metallic oxides are in a ſtate capable of doing injury, being ſtill ſoluble in fat or acid ſubſtances.

Underbaking or imperfect fuſion of the

The underbaking occaſions one of the moſt dangerous defects in pottery; the pieces thus affected have not had ſufficient heat to cauſe the enamel to do more than agglutinate together, and in ſome caſes it even ſtill remains in powder. It is therefore capable of being divided, and taken up by all the liquids with which it may come in contact.

It is eaſy to ſhew the danger to which the public muſt be expoſed in buying thoſe articles at a low price which are called waſte or refuſe and which ought to be carefully thrown away. In vain may it be ſaid that they are uſed daily without any immediate miſchief happening; from the injury being more concealed, it is no leſs deſtructive. It is known that lead and its oxides act inſenſibly on the organs of digeſtion, eſpecially when taken in ſmall quantities: They do not, however, leſs certainly cauſe, at length, emaciation, cholics, convulſions, ſometimes of all parts of the body, with obſtinate diarrhœas; and the wretched people who uſe ſuch veſſels become the victims of their own ignorance, and of the imprudent avarice of the manufacturer.

It would be to the honour of enlightened manufacturers, not to offer to the public pieces which have imperfections beyond a certain degree, and to make this ſacrifice to the good of national commerce, eſpecially as they can avoid the loſs by a greater attention to their materials.

XVIII. Extract

XVIII.

Extract of a Letter from M. JOHN MICHAEL HAUSSMANN, to M. BERTHOLLET, *on the Exiſtence of intermediate Terms of Oxidation.* *

I THINK there are ſufficient grounds for admitting, with you, that there exiſt, in the oxidation of many metallic bodies, intermediate degrees between the *minimum* and the *maximum*.

Exiſtence of intermediate degrees of oxidation of metals.

The firſt example I ſhall cite, is, that of a *minimum* oxide of tin, precipitated from the muriatic ſolution, and diſſolved in an exceſs of cauſtic potaſh; a metallic alkaline ſolution which I have before noticed in my Obſervations on the Red Dye of Adrianople, inſerted in the "*Annales de Chimie*," and alſo in a Memoir on the coloured Oxides of Tin, inſerted in the "*Journal de Phyſique*."

Oxide of tin.

By avoiding any dilution of the muriate of tin, and uſing a very concentrated ſolution of cauſtic potaſh, the mixture diſengages much caloric, part of the tin is precipitated in the metalline ſtate, whilſt the remainder is held in ſolution in an intermediate ſtate of oxidation. This alkaline ſolution is ſo diſoxidant, that it changes the yellow oxide of gold, fixed on cotton, by means of ammonia, to a grey; whilſt a ſimilar yellow pattern underwent no change of colour on being ſteeped in the ſimple liquor of cauſtic potaſh. A like alteration took place on dipping a cotton cloth, which had been previouſly ſtained with the ſolution of gold, and well dried in the alkaline ſolution of tin, which alſo produced the ſame effect on pouring into it the pure ſolution of gold diluted with water.

Experiment. Muriate of tin is in part precipitated metallic and the reſt ſuſpended with intermediate oxidation.

This change of the yellow colour of oxide of gold by the alkaline ſolution of tin, is not the only proof of an intermediate ſtate of oxidation; this liquor poſſeſſes beſides, a property of deſtroying the blackiſh-brown colour of the oxide of manganeſe ſtained upon cotton by an alkaline precipitant.

Other proofs of that intermediate ſtate.

All theſe changes are more rapidly produced, if, prior to the precipitation and ſolution in the cauſtic potaſh liquor, the muriatic ſolution of tin be diluted with ſix or eight parts of water, in which caſe there is no ſenſible diſengagement of caloric, and no tin is precipitated in the metalline ſtate. This ſolution, whoſe oxidation approaches the degree of *minimum*, for the moſt part retains an aqueous tranſparency, without any precipitation of oxide; even when long expoſed to the atmoſpheric air, it does not loſe the property of changing the yellow oxide of gold to a grey colour, or of deſtroying the blackiſh brown tint of oxide of manganeſe, when fixed upon cotton.

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* Annales de Chimie, Vol. LVI. 5.

Oxide of manganeſe.

The oxide of manganeſe is capable of various degrees of oxidation; if a piece of cotton cloth be dipped in the tranſparent ſolution of ſulphate of manganeſe, it will, when dry, retain its original whiteneſs; but on their dipping the ſame cloth in the liquor of carbonated or cauſtic potaſh, it will, after waſhing and expoſure to the atmoſpheric air, be coloured brown; which colour will acquire a deeper ſhade, approximating to black, on being ſteeped for a time in an oxigenated alkaline, muriatic liquor. The oxigenated alkaline liquor, on being for any length of time ſubmitted to the action of the brown precipitate of manganeſe, inſtead of the rag ſteeped therein (which is to diſſolve by means of an increaſed oxidation) will aſſume a purple colour, of greater or leſs tranſparency as the time of their union has been longer or ſhorter.

Other oxides.

There ſeems reaſon, generally, to expect particular reſults from ſubmitting any of the metallic oxides to the action of this oxigenated muriatic alkaline liquor; which might, perhaps, be a means of giving them acid properties, and at the ſame time of proving the gradual oxidation of many metals; this is the more obſervable in white oxide of lead, which becomes gradually coloured by long expoſure to the oxigenated liquor, and being frequently ſtirred.

Muriatic and nitro-muriatic ſolutions of tin, though colourleſs in themſelves, acquire by admixture a vinous tint.

Muriatic and nitro-muriatic ſolutions of tin, well diluted with water, have an aqueous tranſparency, when properly made; but if the two be mixed together, they acquire a fine vinous colour, ſimilar to that of Malaga; this can only ariſe from the oxigen of the nitro-muriatic being in part communicated to the muriatic ſolution of tin.

The addition of ſolution of gold produces a purple dye;

If a ſolution of gold with great exceſs of acid, and diluted with from 130 to 160 parts of water, be gradually poured into the above mixture, ſtirring it all the time, the intenſity of the colour will be increaſed, till at length the liquor becomes

comes of a beautiful purple hue, in which all kinds of goods may be dyed; this may be changed to the tint of peach or lilac bloſſoms, by increaſing the proportions of the nitro muriatic ſolution; or, on the other hand, by cauſing the muriatic ſolutions of tin to preponderate; ſhades of grey will be obtained, deeper or paler in colour, according to the quantity of the ſolution added. Care muſt, however, be taken, in the latter experiment, that too great a proportion of the muriatic liquor of tin be not uſed; for by depriving the oxide of gold of too much of its oxigen, it might be too much diſoxided and precipitated. The precipitate cauſed by ſuch an accident is not altogether void of oxigen, which prevents its gilding cold ſilver, as do the aſhes of burned cloth impregnated with the ſolution of gold. The degree to which the preſervation of the tincture of gold may be carried, muſt depend on the proportions of the two ſolutions of tin, their being more or leſs ſurcharged with acids, and the quality of the ſolution of gold, wherein alſo there ſhould be a very great exceſs of acid.

capable of being changed to the bloom of peaches or lilac;

or even to grey, by proportioning the quantity of the two ſolutions of tin.

The precipitate of the oxide of gold will never gild ſilver, without the aſſiſtance of heat.

The purple tincture of gold, though of the moſt perfect tranſparency, is decompoſed by expoſure to a ſtrong heat, and throws down what is known by the name of "*Purple of Caſſius*," whoſe beauty depends on the quantity of nitro-muriatic ſolution of tin made uſe of. The latter, however, if mixed alone with the ſolution of gold, without the preſence of muriate of tin, produces no alteration of colour, and, if the mixture be not too much weakened with water, is a very long time before it gives a precipitate.

Purple powder of Caſſius.

The purple tincture of gold, is, properly ſpeaking, nothing more than the powder of caſſius, held in ſolution by means of the oxigen of the nitro-muriatic liquor of tin; and there is every reaſon to believe, that in the powder of caſſius, the oxide of gold is in ſome way combined with the oxide of tin, which, by tranſmitting to it its own origin, during its fixation upon porcelain, prevents it, I think, from returning to its metallic ſtate. I find a difficulty in ſubſcribing to the opinion of Dr. Richter, of Berlin, who, in a memoir (which I have not read) attempts to prove, by mathematical demonſtration, that the crimſon-coloured gold on porcelain is in the metallic ſtate.

Dr. Richter's opinion of the crimſon-coloured gold upon porcelain.

The purple tincture of gold might be advantageouſly employed in dying ſilks, without greatly enhancing the price. The

Purple tincture of gold ſurpaſſes all others in dying ſilks.

The colour obtained from it ſurpaſſes all others in duration, ſince nothing leſs than combuſtion can deſtroy it. It is neceſſary, however, to leave the ſilk a long time in this dye; and the depth of the ſhade will be in proportion to the number of times the article is dipped; it muſt be well wrung, rinced, and dried, between each immerſion.

The gradation of ſhades already noticed are indications of a gradual oxidation.

The gradation of colours produced by mixture of the nitro-muriatic, and muriatic ſolutions of tin, being much weakened by dropping ſolution of gold in a great exceſs of acid, conſiderably diluted with water, into the mixture, ſeems to me to indicate a gradual oxidation. The acetic ſolution of iron proves the ſame truth; for on being expoſed to the atmoſpheric air, or to the contact of oxigen gas, it gradually changes from a ſea green to a reddiſh yellow colour. I have ſhewn, in a memoir on the alkaline Tincture of Mars of Stahl, that ſulphate of iron may be ſuper-oxigenated, and alſo loſe its exceſs of oxigen by the action of light. On mixing concentrated ſulphuric acid with nitric ſolution of iron, I obtained, after the nitric acid was evaporated, by leaving the reſiduum to imbibe the moiſture of the air for ſeveral months; cryſtals of ſuper-oxigenated ſulphate of iron, which were at firſt diſtinguiſhable by their whiteneſs from ſulphate of alumine; but the action of the light gradually tinged their ſurface with a yellow colour; their original whiteneſs, however, might, by a gentle waſhing, be reſtored at pleaſure. Super-oxigenated ſulphate of iron, of nearly an equal degree of whiteneſs, may in like manner, be obtained by precipitating nitrate of iron, and diſſolving the precipitate, edulcorated and freed from water, gradually in ſulphuric acid, which, if well concentrated, will produce cryſtals of ſuper-oxigenated ſulphate of iron without evaporation. This ſalt poſſeſſes an incomparable degree of aſtringency.

Sulphate of iron loſes its exceſs of oxigen by ex-

The fact that linens printed with acetate of iron are liable to become rotten, a proof of the gradual tranſmiſſion of oxigen.

The progreſs of the tranſmiſſion of oxigen is more manifeſt on linen, ſimply printed with acetate of iron and madder, which muſt be a long time expoſed in the air to bleach, unleſs the artificial means of bleaching be adopted. The printed part of the linen frequently periſhes, bearing the appearance of having been cut with a ſharp inſtrument, or burned with concentrated acid; this, it ſhould ſeem, muſt proceed from the action of the oxigen contained in the coloured oxide of iron, continually repleniſhed from the atmoſpheric air.

It is not among minerals alone that ſubſtances are found which are gradually oxided, and by intermediate degrees.

Indigo affords an inſtance that vegetable and animal bodies offer ſimilar proofs; for any ſolution of indigo (excepting the ſulphate of indigo) will, on diſoxidation, or on having its oxigen reſtored, paſs through all the degrees of ſhade, from blueiſh green to very yellow olive, preſerving in the mean time the ſame quantity of indigo in ſolution. The beauty and ſtability of the colours, either for dying or painting, will chiefly depend on the degree of oxidation. On ſome other occaſion, Sir, I ſhall write to you more amply on this ſubject.

Vegetable and animal bodies furniſh ſimilar caſes with minerals, of gradual oxidation.

SCIENTIFIC NEWS.

Mémoires de l'Académie impériale des Sciences, &c. Memoirs of the imperial Academy of Sciences, Literature, and fine Arts, of Turin, for the Years 12 and 13, 2 Vols. Quarto. 1805. *Turin.*

WHEN the Royal Academy of Turin aſſumed the name of Imperial, in conſequence of Piedmont being annexed to France, the number of academicians was increaſed, to form a new claſs, that of literature and the fine arts. Of the two volumes publiſhed, one is appropriated to the labours of this claſs, the other to that of the phyſical and mathematical ſciences.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

The latter is compiled by the ſecretary, Mr. Vaſſali Eandi, who firſt mentions the changes that have taken place in the liſt of academicians, next the various papers that have been read at their meetings, and then the books and other articles preſented to the ſociety. Theſe liſts are followed by a well written account of the labours of the academy up to the year 1805, which occupies 250 pages. After this follow the different memoirs.

1. Deſcription and uſe of a new portable barometer, for meaſuring heights and depths, with obſervations made with this inſtrument in the circles of Turin and Saluzzo. This inſtrument, of which a figure is given, was invented by the ſecretary; who has ſubjoined to his paper ſome very curious hiſtorical notes on the places where his obſervations were made.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

2. Account of a waterſpout that occurred in the territory of Revel, in the circle of Saluzzo, March 27, 1798, with remarks on the cauſe of the phenomenon, by the ſame.

3. On the different capacities for conducting heat aſcertained by experiment in different articles uſed for clothing, by J. Sennebier.

4. Of a new ſpecies of hawkweed, *crepis*, to which are added ſome cryptogamiæ of Piedmont, by J. Baptiſt Balbis. A figure of this plant, which Mr. B. calls *crepis ambigua*, is given. Among the cryptogamiæ are the following new ſpecies, *mucor floſculatus*, *peziza amentacea*, *lichen nivalis*. Theſe likewiſe are figured.

5. Experiments on the effects of the nitric and oxigenated muriatic acid, employed topically in the treatment of various diſeaſes, by Mr. Roſſi. Mr. R. gives an account of the cure of ſeveral gangrenous ulcers, venereal buboes, and even contagious carbuncles, cured by the application of theſe acids.

6. Meteorological obſervations made during the ſolar eclipſe on the 30th of Jan. 1805, at the obſervatory of Turin, with reflections on them, by Ant. Mar. Vaſſali Eandi.

7. On a ſpecies of caſſia, that may be ſubſtituted for the ſenna of the ſhops, by Mr. Bellardi. This is the *caſſia marilandica*, which Mr. B. would call *ſuccedanea*, becauſe, according to him, it may ſupply the place of the *caſſia lanceolata*.

8. Inquiries into the nature of the galvanic fluid, by A. M. Vaſſali Eandi.

9. On the mines of plumbago in the departments of the Sture and the Po, by Mr. Bonvoiſin.

10. Attempts to improve nut oil, by the ſame. Mr. B. points out a method of purifying this oil, and rendering it as fit for lamps as other fine oils.

11. Examination of the action of the galvanic fluid on different gaſes, by J. A. Giobert.

12. An anatomical and phyſiological eſſay on the lymphatic glands, by profeſſor Roſſi.

13. Solution of a problem depending on the theory of permutations and combinations, by profeſſor Balbo.

14. Explanation of the circumſtance of a fiſh being occaſionally found with prickles in the river of the 27th military

diviſion, by M.r Giorna. The fiſh is the *cyprinus idus*; the male only has prickles, and loſes them after ſpawning time.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

15. A chemico-medical eſſay on the pulmonary conſumption, by Joſ. Hyac. Rizzetti. The principal ſubject of this eſſay is the nature of the matter expectorated.

The following papers are by foreign members.

1. Memoir on the uſe of varying the conſtant quantity in ſumming up equations with variable coefficients, by Dr. Brunacci.

2. A ſyſtematical enumeration of the coleoptera found in the territory of Saluzzo, with obſervations, by Law. Ponza. To this catalogue are annexed two plates, containing the following new ſpecies. *Coccinella numeralis,—c. obſoleta,—curculio ſpinoſus,—c. dubius,—c. rugoſus,—cerambyx præuſtus,—c. melanocephalus,—chryſomela melanocephala,—ch. variegata,—ch. pretioſa,—ch. luctuoſa,—ſcarabæus rufeſcens,—cantharis impreſſifrons,—attelabus funereus,—dytiſcus ſilphoides,—tenebrio rufus,—birrhus roſſii,—carabus attenuatus,—c. metallicus,—c. roſſii,—forficula bipunctata,—ſilpha ſinuata,—ſ. ſcabra.*

3. On the motion of the hairs of the *hypnum adiantoides*, by Palamedas de Suffren. Parts endued with irritability had already been obſerved in the hairs of ſome moſſes. Mr. De S. has found it in thoſe of the *h. a.* and deſcribes all the ſingularities of the phenomenon. This paper is accompanied with a plate.

4. Of a reſin employed by the bee in conſtructing its combs. By Fr. Mouxy Deloche.

5. Entomological obſervations; by Mr. Diſderi. Mr. D. firſt ſketches the hiſtory of the ſilkworm; and then proceeds to certain hymenoptera, chiefly of the *genera tenthredo, ichneumon, ſphex, et veſpa.*

6. Specimen of the fungi of the vale of Piſa, by Hugh Camino. The new ſpecies are figured on three plates. They are Agaricus elatior: a. miniatus: a. pezizoides: a. aſtroſanguineus: a. tricolor: Boletus ſcobinaceus: Helvella grandis: h. reflexa: h. inflata: Peziza aebracea: p. pyriformis: Reticularia roſea: Mucor fruticuloſus.

7. Obſervations on the native gold found among ſand, by Lew. Boſſi, of Milan.

To

Barometer.

MY correſpondent from Edinburgh is reminded, with regard to his project for a barometer, that no enlargement or diminution of the bore will make the leaſt difference in the ſcale of the common barometer, conſiſting of a tube or veſſel, cloſed above, and having its lower end open, and communicating with a baſon of mercury of conſiderable diameter.

Subdiviſion of an arc by wheel and chain.

The contrivance, received ſome time ago from T. I. for making an aſtronomical inſtrument, in which the angular quantities ſhall be meaſured by the communication of a chain, ſtrap, or ſtring, poſſeſſes ſo much ingenuity and promiſe, that it has exerciſed the heads and hands of a number of eminent men. Among theſe are Robert Hooke, for a quadrant; Muſchenbroeck, for a pyrometer, and many operative men, ſuch as Siſſon and others, for theodolites and quadrants.—Where the intention of the inſtrument is ſimply to magnify the motion, without any particular attention to preciſion, the contrivance has a happy effect; particularly in public lectures, where a number of ſpectators may obſerve the ſame effect at the ſame time. It is likewiſe cheap, and may be carried into effect in ſituations where the uſe and application of more accurate apparatus cannot be referred to.

It cannot be made very exact.

A ſlight attention to the ſubject will ſhew that all contrivances of the kind here alluded to muſt be conſiderably inaccurate. For they demand, 1, that the wheels ſhould be very truly circular: 2, and free from all dirt and impurity: 3. that they be well centered: 4. that the chain or ſtring ſhould be every where of the ſame thickneſs: 5, and its tenſion in all poſitions alike, &c. &c. If the quantity of error, taken at a minimum, which muſt ariſe from theſe and other cauſes, be attended to, it will be found that a ſimple diviſion of an arc (ſubdivided by a ſcrew or a nonius) and examined or read off by a ſmall magnifier, will afford greater preciſion; even when the work is performed by a careful deſigner, who is no mathematical inſtrument maker. It is certain that much greater delicacy and preciſion may be had in the diviſion of mathematical inſtruments by the patient diligence of a cultivator of practical mechanics than is generally ſuppoſed.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO *VOL. XIII.*

ARTICLE I.

On the Saline Effloreſcences upon Walls; Salivary Concretions; Deflagration of Mercury by Galvaniſm; Biliary Calculi; and the freezing Point of Spermaceti. By JOHN BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR,

Examination of two ſpecimens of effloreſcence found upon walls.

IN the third and ſixth volumes of your Journal you have inſerted an account of ſome experiments that I performed on the ſaline effloreſcences found upon walls. I have lately had an opportunity of examining two other ſpecimens, of which I now ſend you the particulars. The firſt was obtained in conſiderable quantity from the inner walls of a warehouſe that had been erected about twenty years. By a ſeries of ſimple experiments, which it is unneceſſary to detail at full length, I found it to be a ſulphate of ſoda, which, as in the former caſes, ſeemed to exiſt in a ſtate of almoſt perfect purity. The circumſtances attending the ſecond of theſe effloreſcences were more ſingular. It was given me by a friend who had ſcraped it from off the ſtones which are ſituated on the inſide of the weſt aiſle of York Minſter. My friend, on whoſe ac-

The firſt was ſulphate of ſoda:

The ſecond was ſcraped from the

Face of the ſtone withinſide York Minſter, and not from the mortar.

curacy I place the fulleſt confidence, expreſsly ſtated, that it was taken from the ſurface of the ſtone itſelf, and not from the joints, or any part that had been covered with mortar. It exiſted there in large quantity, and was diſpoſed in the form of projecting ſpiculæ. Upon ſubjecting it to the uſual trials, I found it to be a very pure ſulphate of magneſia. In order to aſcertain with preciſion the degree of its purity, I prepared a quantity of the ſulphate of magneſia, by uniting together its conſtituent parts. This artificial ſalt, and the ſalt from York, after being cryſtallized, were expoſed for ſome time to the ſame degree of heat, and when all the water of cryſtallization appeared to be expelled, equal weights of them were diſſolved in equal weights of water: 100 grains of theſe ſolutions had the muriate of barytes reſpectively added, until no farther precipitation was produced, when it appeared that exactly the ſame weight of barytes was neceſſary to ſaturate each ſolution. The portions of precipitated ſulphate of barytes were collected and dried, and when examined by a nice balance, exhibited ſcarcely any perceptible difference in weight; they each amounted to 7.9 grains. A ſimilar proceſs being adopted with reſpect to the common Epſom ſalt of the ſhops, the precipitate was found to be 7.35 grains only. Before I quit this ſubject, I may remark that another friend, in viſiting the cathedral at Tewkeſbury, noticed a ſaline effloreſcence on the inſide of ſome part of that building; he collected a portion of it, intending to give it me for examination; but it was accidentally loſt. Perhaps ſome of your readers, who reſide in that neighbourhood, may be induced to examine it, and tranſmit the reſult to your Journal. I confeſs myſelf totally unable to explain the production of the ſulphate of magneſia on the ſurface of a freeſtone, ſuch as, I believe, forms the body of York Minſter.

It was ſulphate of magneſia, very pure.

Qu. Whence came the magneſia?

Account of a concretion in the ſalivary ducts.

Among the ſolid concretions which are formed in different parts of the human body, thoſe from the ſalivary ducts are occaſionally met with. I lately procured one of theſe ſubſtances, of which I will give you a brief account. It was a cylinder, pointed at one end, of half an inch in length, and ſomewhat more than $\frac{1}{12}$ of an inch in diameter; it weighed $1\frac{1}{2}$ gr. It was white and ſmooth on the outſide, and its internal fracture did not exhibit any marks of regular organization. To half a grain of the concretion a few drops of diluted muriatic acid were added; no effervefcence was excited. By the application of a gentle heat the whole was diſſolved, except a few films that ſwam in the fluid. A copious precipitation was produced in this ſolution by pure ammonia, but none by the carbonate of ammonia. A part of the muriatic ſolution was evaporated; the reſidue was not ſoluble in water, but was ſpeedily re-diſſolved by the muriatic acid. The muriatic ſolution, ſaturated with the carbonate of ammoniac, had a precipitation produced by the oxalate of ammoniac. It appears therefore that the concretion conſiſted of the phoſphate of lime, mixed with a little animal matter, probably coagulated albumen; it did not contain any carbonate of lime, and its component parts appeared not to poſſeſs any regularly organized ſtructure. M. Fourcroy * and Dr. Thomſon † have examined ſimilar bodies, and agree in conſidering the earthy matter to be the phoſphate of lime; we may therefore reaſonably conclude that this ſubſtance always compoſes the earthy part of the ſalivary concretions. I am diſpoſed, however, to differ from theſe diſtinguiſhed chemiſts in my idea reſpecting the nature of the animal matter which enters into their compoſition; M. Fourcroy conſiders it as conſiſting of a ſpecies of mucilage, while Dr. Thomſon deſcribes it as "a membranous ſubſtance, which retains the ſhape of the concretion after the ſolution of the phoſphate." This was certainly not the caſe with the one which I examined. I am diſpoſed to conſider the animal matter as coagulated albumen, rather than mucus, in conſequence of its inſoluble nature, and the greater facility with which it would on this account be detained by the phoſphate of lime.

It was phoſphate of lime chiefly,

—with coagulated albumen.

The power which the electric fluid poſſeſſes, when generated by the galvanic apparatus, of burning metallic plates, affords one of the moſt beautiful experiments of which the ſcience of chemiſtry can boaſt. All the metals have by this means been ſubjected to combuſtion, except mercury, which, owing to its fluidity, is incapable of being formed into thin laminæ. ‡ I have, however, been fortunate enough to accompliſh this object, and that by the moſt ſimple method.

Mercury has not heretofore been deflagrated by galvaniſm.

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* Syſteme, IX. 368. † Chemiſtry, IV. 658.

‡ Thomſon's Chemiſtry, I. 125.

Experiment in which this was effected.

I was performing ſome experiments with Mr. Richard Dalton, an ingenious lecturer in natural philoſophy of this place, with a pile compoſed of 60 pair of ſix-inch plates of zinc and copper, when it occurred to me to place a minute globule of mercury in an iron ſpoon, reſting on the top of the pile, and to approach to it a thick iron wire connected with the other end of the apparatus; the effect was, that a brilliant ſtar of light was produced from the mercury, attended with a crackling noiſe and a copious emiſſion of ſparks; the mercury was found converted into the black oxide.

The dark coloured particles of gall-ſtones do not appear to be inſpiſated reſin of the bile.

The moſt common ſpecies of biliary calculus is that compoſed of the peculiar cryſtalline matter, which in ſome of its properties reſembles ſpermaceti, through which are interſperſed a number of dark coloured particles, that are ſuppoſed to conſiſt of hardened bile. This is the idea entertained by M. Fourcroy,* and the one which I adopted, when I made the experiments on this ſubject which are related in the fourth volume of your Journal. I have, however, ſince that time been diſpoſed to alter my opinion; in two ſpecimens of the biliary calculi, which I examined, after ſeparating the cryſtalline matter by alcohol, I was unable to diſſolve the dark coloured particles by any menſtruum which I applied to them; they imparted a yellowiſh tinge to water and other fluids, but the great bulk of their ſubſtance remained unchanged. It is, I conceive, not probable that the mere inſpiſſation of the reſin of the bile could ſo far alter its properties. I mention this circumſtance principally with a view of attracting the attention of any of your readers who may be in poſſeſſion of a number of gall-ſtones, ſo as to aſcertain whether the untractable nature of theſe particles is a general property of the cyſtic-adipo-bilious concretions, or ſomething peculiar to the ſpecimens upon which I experimented.

Melting point of ſpermaceti. Repetition of experiment confirms that it is a little above 112°.

I ſhall conclude this miſcellaneous letter with ſome remarks upon the melting point of ſpermaceti. In the paper to which I have already referred, I mentioned the diverſity of opinion that had been entertained on this ſubject, and afterwards ſtated that my own experience induced me to fix it at the 112th degree. Dr. Thomſon, in the firſt volume of his Chemiſtry, fixes the melting point at 133°,† while in the fourth he ſtates it to be 112°, upon the authority of my paper.* Yet in his anſwer to the Edinburgh reviewers, he has mentioned this eſtimate of the melting point of ſpermaceti as one of his acknowledged errors, and upon the authority of Dr. Gibbes, fixes it at 115°. This circumſtance determined me to repeat the experiment; I employed a very delicate thermometer, and uſed every requiſite precaution; the reſult was that the inſtrument deſcended to *a little above the* 112*th degree*, and remained ſtationary until the ſubſtance was become ſolid. I may add that Dr. Irvine, in ſome experiments related in the ninth vol. of your Journal, fixes the point at 113°, which agrees ſo nearly with my obſervations, as to afford me an additional confidence in their accuracy.

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* Syſteme, X. 591

† Page

I am, Sir,
Your obedient ſervant,
JOHN BOSTOCK.

Liverpool, April 9, 1806.

II.

Inveſtigation of the Temperature at which Water is of greateſt Denſity, from the Experiments of Dr. Hope on the Contraction of Water by Heat at low Temperatures. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

Reference to a former communication, in which the maximum denſity of water was taken at 32°.

IN your Journal for February 1805 was inſerted a letter of mine containing certain facts relative to the ſubject of my preſent communication, which led me to diſbelieve the common opinion that water is denſeſt at 40°, and inclined me to think it is at 32°. Since that time my attention has again been turned to the ſubject; ſome ſmall but immaterial corrections of the facts have been made and additional ones obtained, by which I have been enabled to demonſtrate, at leaſt to my own ſatisfaction, that the temperature at which water is of greateſt denſity is at or near 36° of Fahrenheit. The reſults have lately been communicated to the Mancheſter Society, and may perhaps

Preſent inference that it is a 36°.

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* Page 512.

perhaps appear in a future volume. My preſent object is to ſhew that the reſults of Dr. Hope's experiments are explicable on the ſuppoſition of water being denſeſt at 36°, but on no other.

Obſervations on the expanſions of water on each ſide of its point of greateſt denſity.

Dr. Hope and myſelf concur in the opinion that water is denſeſt at ſome one point of temperature, and that above and below that point it expands alike by heat and cold in a gradually increaſing manner. De Luc was the firſt to obſerve that the expanſion is the ſame quantity for the ſame number of degrees, whether of increaſe or diminution of temperature; the remarkable fact was extended by my former experience from a range of 8° to 25° or more, above and below the ſtationary point. I have lately examined this fact with greater attention to preciſion than formerly, and find that it is accurate, except that the expanſion for degrees *below* the ſtationary point is always ſomewhat more than for a correſponding number of degrees *above* the ſaid point. Thus, water is ſtationary in a glaſs thermometer at 42°; if heated to 75° by the mercurial ſcale, it expands very conſiderably; if plunged into a frigorific mixture of 13°, it falls to 42°, and then expands again to the ſame point of 75°, at which it remains ſtationary as long as continued in the mixture. It may be remarked too, that congelation rarely if ever takes place in the bulb, when the mixture is not below 15°, which may eaſily be procured by putting ſnow into water ſaturated with common ſalt. Hence we ſee that 29° below, afford the ſame expanſion as 33° above the ſtationary point. This, I imagine, is occaſioned by the error attached to the equal diviſion of the mercurial ſcale. For a ſmall number of degrees, however, we may admit that the expanſions for correſponding intervals above and below are equal; hence we obtain the following table of correſponding temperatures at which water is of the ſame denſity.

	Suppoſing greateſt denſity at 40°			at 36°	
	39° and	41°		35° and	37°
	38	42		34	38
Correſponding denſities will be at	37	43	Correſponding denſities will be at	33	39
	36	44		32	40
	35	45		31	41
	34	46		30	42
	33	47		29	
	32	48		28	44

The author and Dr. Hope concur that water may continue fluid below its freezing point.

Dr. Hope alſo admits with me the fact that water ſubjected to be cooled without agitation in a frigorific mixture, uſually deſcends ſeveral degrees below the freezing point, and ſtill retains its liquidity. Though it is eaſy to obtain water in a glaſs bulb 20 or 25° below freezing, I could never cool water in an open jar more than 10 or 11° below freezing, agreeable to the experience of Sir Charles Blagden. But I find water in ſuch circumſtances will admit of being cooled to 25°, and the bulb of a thermometer to be immerſed and withdrawn ſeveral times, without freezing.

We come now to the experiments of Dr. Hope.

Experiment I.

Dr. Hope's experiment quoted.

A jar eight inches deep and 4½ in diameter, filled with water of 32°, and placed on a table, &c. Air 60—62°. Two thermometers inſerted, one at the top, another at the bottom.

Top Thermometer.		Differences.	*Bottom Thermometer.*	Differences.
	32°	0	32°	
		1+		2+
In 10 min.	33+		34+	
		2.5—		3—
30 ——	35.5		37	
		1.5		1+
50 ——	37		38+	
		1		0
1 hour ——	38		38+	
		4		.25—
1 10 ——	42		38.25	
		2		1.75
1 30 ——	44		40	&c.

Inference that the water deſcends as it acquires heat.

In the firſt interval of 10 minutes we obſerve the bottom thermometer to have gained 2°+, and the top only 1°+; the former has the heat which enters directly, together with the heat which deſcends by the ſide of the veſſel; the latter has only the heat which enters directly, and as theſe are nearly as one to two, we may infer that the acquiſition of direct heat, and heat by deſcent, are nearly equal in the bottom thermometer during that interval.

In the next interval of 20 minutes we obſerve the bottom thermometer gains 3°—, and the top 2°,5—. Here we ſee the aſcending current ſtill continues, but has produced little effect, having not added more than half a degree to the temperature.

—until the temperature rather exceeds 36°.

During the next 20 minutes the top gains 1°.5, the bottom only 1°+. In this interval we may obſerve the current has turned, but not yet acquired much force. The point of greateſt denſity muſt therefore have exiſted at the laſt obſervation or near it: the mean of 35°.5 and 37° is 36¼° for the required point, as deduced from this experiment.

After which the heated water aſcends.

In 10 minutes more the top gains 1°, and the bottom little or nothing; here we find the aſcending current has become ſuch as to manifeſt its influence very ſenſibly.

In the next 10 minutes the top gains 4°, and the bottom only ,25; here the aſcending current has become quadruple what it was 2° below; becauſe the farther the temperature is raiſed above the ſtationary point, the more powerful is the force of aſcent ariſing from the ſame interval of temperature.

Theſe facts do not agree with the ſuppoſed maximum denſity at 39° or 40°.

It would be in vain to attempt to reconcile the above experiment with the opinion that water is denſeſt at 39° or 40°. At the very moment when the mean temperature of the water is 39°, we obſerve the *aſcending* current the moſt active, when it ought to have been *deſcending* or imperceptible.

The effect is not modified by the table or ſupport.

I once imagined that the experiment might be explained on the ſuppoſition of 32° being the point of greateſt denſity; that the ſudden increaſe of temperature at the bottom aroſe from the heat of the table, and that the coheſion of the particles of water prevented their aſcent under the propulſion of ſo ſmall a force; but having procured a large glaſs jar which could be ſuſpended, I found the ſame order of differences nearly as when placed on a table, and was therefore obliged to abandon that explanation.

Intending to ſend the remainder of this inveſtigation for a future number, I remain

Your friend,

JOHN DALTON.

Mancheſter, April 14, 1806.

III.

Account of a Series of Experiments, ſhewing the Effects of Compreſſion in modifying the Action of Heat. By Sir James Hall, *Bart. F. R. S. Edinburgh.*

(Concluded from Page 328.)

Section III.

Experiments made in Tubes of Porcelain.—Tubes of Wedgwood's Ware.—Methods uſed to confine the carbonic Acid, and to cloſe the Pores of the Porcelain in a horizontal Apparatus.—Tubes made with a View to theſe Experiments.—The vertical Apparatus adopted.—View of Reſults obtained, both in Iron and Porcelain.—The Formation of Lime-ſtone and Marble.—Inquiry into the Cauſe of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking—Experiments with Porcelain Tubes proved to be limited.

Set of experiments in tubes of porcelain.

WHILE I was carrying on the above-mentioned experiments, I was occaſionally occupied with another ſet, in tubes of porcelain. So much, indeed, was I prepoſſeſſed in favour of this laſt mode, that I laid gun-barrels aſide, and adhered to it during more than a year. The methods followed with this ſubſtance differ widely from thoſe already deſcribed, though founded on the ſame general principles.

I procured from Mr. Wedgwood's manufactory at Etruria, in Staffordſhire, a ſet of tubes for this purpoſe, formed of the ſame ſubſtance with the white mortars, in common uſe, made there. Theſe tubes were fourteen inches long, with a bore of half an inch diameter, and thickneſs of 0.2; being cloſed at one end (figs. 9, 10, 11, 12, 13.) *Pl.* XI.

They were cloſed at one end and the other aperture was ſtopped with glaſs of borax.

I propoſed to ram the carbonate of lime into the breech (*Fig.* 9. A); then filling the tube to within a ſmall diſtance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the ſhops (borat of ſoda) previouſly reduced to glaſs, and then pounded; to apply heat to the muzzle alone, ſo as to convert that borax into ſolid glaſs; then, reverſing the operation, to keep the muzzle cold, and apply the requiſite heat to the carbonate lodged in the breech.

Difficulties of this proceſs.

I thus expected to confine the carbonic acid; but the attempt was attended with conſiderable difficulty, and has led to the employment of various devices, which I ſhall now ſhortly enumerate, as they occurred in the courſe of practice. The ſimple application of the principle was found inſufficient, from two cauſes: Firſt, The carbonic acid being driven from the breech of the tube, towards the muzzle, among the pores of the pounded ſilex, eſcaped from the compreſſing force, by lodging itſelf in cavities which were comparatively cold: Secondly, The glaſs of borax, on cooling, was always found to crack very much, ſo that its tightneſs could not be depended on.

And the method of partly obviating them.

To obviate both these inconveniences at once, it occurred to me, in addition to the firſt arrangement, to place ſome borax (*Fig.* 10. C) ſo near the breech of the tube, as to undergo heat along with the carbonate (A); but interpoſing between this borax and the carbonate, a ſtratum of ſilex (B), in order to prevent contamination. I truſted that the borax in a liquid or viſcid ſtate, being thruſt outwards by the expanſion of the carbonic acid, would preſs againſt the ſilex beyond it (D), and totally prevent the elaſtic ſubſtances from eſcaping out of the tube, or even from wandering into its cold parts.

In ſome reſpects, this plan anſwered to expectation. The glaſs of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued viſcid maſs in the loweſt red-heat; and as the ſtreſs in theſe experiments begins only with redneſs, the borax being heated at the ſame time with the carbonate, becomes united and impervious, as ſoon as its action is neceſſary. Many good reſults were accordingly obtained in this way. But I found, in practice, that as the heat roſe, the borax began to enter into too thin fuſion, and was often loſt among the pores of the ſilex, the ſpace in which it had lain being found empty on breaking the tube. It was therefore found neceſſary to oppoſe ſomething more ſubſtantial and compact, to the thin and penetrating quality of pure borax.

Bottle glaſs was found much preferable to pure borax for the purpoſe of reſtraining the carbonic acid.

In ſearching for ſome ſuch ſubſtance, a curious property of bottle-glaſs occurred accidentally. Some of this glaſs, in powder, having been introduced into a muffle at the temperature of about 20° of Wedgwood; the powder, in the ſpace of about a minute, entered into a ſtate of viſcid agglutination, like that of honey, and in about a minute more, (the heat always continuing unchanged), conſolidated into a firm and compact maſs of *Reaumur's porcelain**. It now appeared, that by placing this ſubſtance immediately behind the borax, the penetrating quality of this ſalt might be effectually reſtrained; for, Reaumur's porcelain has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of conſolidation, the pounded bottle-glaſs ſhrunk, ſo as to leave an opening between its maſs and the tube, through which the borax, and, along with it, the carbonic acid, was found to eſcape. But the object in view was obtained by means of a mixture of pounded bottle-glaſs, and pounded flint, in equal parts. This compound ſtill agglutinates, not indeed into a maſs ſo hard as Reaumur's porcelain, but ſufficiently ſo for the purpoſe; and this being done without any ſenſible contraction, an effectual barrier was oppoſed to the borax; (this arrangement is ſhewn in *Fig.* 11.); and thus the method of cloſing the tubes was rendered ſo complete, as ſeldom to fail in practice †. A ſtill further refinement upon this method was found to be of advantage. A ſecond ſeries of powders, like that already deſcribed, was introduced towards the muzzle, (as ſhewn in *Fig.* 12). During the firſt period of the experiment, this laſt-mentioned ſeries was expoſed to heat, with all the outward half of the tube *(a b)*; and by this means, a ſolid maſs was produced, which remained cold and firm during the ſubſequent action of heat upon the carbonate.

Improvements on this method.

Remedy for poroſity in the earthen tubes.

I ſoon found, that notwithſtanding all the above-mentioned precautions, the carbonic acid made its eſcape, and that it pervaded the ſubſtance of the Wedgwood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating ſtate of fuſion, applied to the inſide of the tube; and that the pores of the porcelain might thus be cloſed, as thoſe of leather are cloſed by oil, in an air-pump. In this view, I rammed the carbonate into a ſmall tube, and ſurrounded it with pounded glaſs of borax, which, as ſoon as the heat was applied, ſpread on the inſide of the large tube, and effectually cloſed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffles, (the arrangement juſt deſcribed being repreſented in *Fig.* 13.)

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* In the ſame temperature, a maſs of the glaſs of equal bulk would undergo the ſame change; but it would occupy an hour.

† A ſubſtance equally efficacious in reſtraining the penetrating quality of borax, was diſcovered by another accident. It conſiſts of a mixture of borax and common ſand, by which a ſubſtance is formed, which, in heat, aſſumes the ſtate of a very tough paſte, and becomes hard and compact on cooling.

Beſt material for tubes.

I was thus enabled to carry on experiments with this porcelain, to the utmoſt that its ſtrength would bear. But I was not ſatisfied with the force ſo exerted; and hoping to obtain tubes of a ſuperior quality, I ſpent much time in experiments with various porcelain compoſitions. In this, I ſo far ſucceeded, as to produce tubes by which the carbonic acid was in a great meaſure retained without any internal glaze. The beſt material I found for this purpoſe, was the pure porcelain-clay of Cornwall, or a compoſition in the proportion of two of this clay to one of what the potters call *Corniſh-ſtone*, which I believe to be a granite in a ſtate of decompoſition. Theſe tubes were ſeven or eight inches long, with a bore tapering from 1 inch to 0.6. Their thickneſs was about 0.5 at the breech, and tapered towards the muzzle to the thinneſs of a wafer.

Improvement by placing the tube vertically.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally, as before. By obſerving the thin ſtate of borax whilſt in fuſion, I was convinced, that it ought to be treated as a complete liquid, which being ſupported in the courſe of the experiment from below, would ſecure perfect tightneſs, and obviate the failure which often happened in the horizontal poſition, from the falling of the borax to the lower ſide.

Particular deſcription of the proceſs.

In this view, (fig. 16.) I filled the breech in the manner deſcribed above, and introduced into the muzzle ſome borax (C) ſupported at the middle of the tube by a quantity of ſilex mixed with the bottle glaſs (B). I placed the tube, ſo prepared, with its breech plunged into a crucible filled with ſand (E), and its muzzle pointing upwards. It was now my object to apply heat to the muzzle-half, whilſt the other remained cold. In that view, I conſtructed a furnace (figs. 14 and 15.) having a muffle placed vertically (*c d*,) ſurrounded on all ſides with fire (*e e*), and open both above (at *c*), and below (at *d*). The crucible juſt mentioned, with its tube, being then placed on a ſupport directly below the vertical muffle, (as repreſented in fig. 14. at F) it was raiſed, ſo that the half of the tube next the muzzle was introduced into the fire. In conſequence of this, the borax was ſeen from above to melt, and run down in the tube, the air contained in the powder eſcaping in the form of bubbles, till at laſt the borax ſtood with a clear and ſteady ſurface like that of water. Some of this ſalt being thrown in from above, by means of a tube of glaſs, the liquid ſurface was raiſed nearly to the muzzle, and, after all had been allowed to become cold, the poſition of the tube was reverſed; the muzzle being now plunged into the ſand, (as in fig. 17.) and the breech introduced into the muffle. In ſeveral experiments, I found it anſwer well, to occupy great part of the ſpace next the muzzle, with a rod of ſand and clay previouſly baked, (fig. 19. K K), which was either introduced at firſt, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid ſtate. In many caſes I aſſiſted the compactneſs of the tube by means of an internal glaze of borax; the carbonate being placed in a ſmall tube, (as ſhewn in fig. 18.)

Theſe devices anſwered the end propoſed. Three-fourths of the tube next the muzzle was found completely filled with maſs, having a concave termination at both ends, (*f* and *g* fig. 17, 18, 19.) ſhewing that it had ſtood as a liquid in the oppoſite poſitions in which heat had been applied to it. [illegible] a degree of tightneſs indeed was obtained in this [illegible] I found myſelf ſubjected to an unforeſeen ſource of [illegible] number of the tubes failed, not by exploſion, but by [illegible] formation of a minute longitudinal fiſſure at the breech, through which the borax and carbonic acid eſcaped. I ſaw that this aroſe from the expanſion of the borax when in a liquid ſtate, as happened with the fuſible metal in the experiments with iron-barrels; for, the crevice here formed, indicated the exertion of ſome force acting very powerfully, and to a very ſmall diſtance. Accordingly, this ſource of failure was remedied by the introduction of a very ſmall air-tube. This, however, was uſed only in a few experiments.

Effect of expanſion in the fuſed borax upon the tubes.

In the courſe of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above deſcribed, amounting, together with thoſe made in gun barrels, to one hundred and fifty-ſix. In an operation ſo new, and in which the apparatus was ſtrained to the utmoſt of its power, conſtant ſucceſs could not be expected, and in fact many experiments failed, wholly or partially. The reſults, however,

Theſe experiments were upon the whole ſucceſsful.

upon the whole, were ſatisfactory, ſince they ſeemed to eſtabliſh ſome of the eſſential points of this inquiry.

Theſe experiments prove, that, by mechanical conſtraint, the carbonate of lime can be made to undergo ſtrong heat, without calcination, and to retain almoſt the whole of its carbonic acid, which, in an open fire, at the ſame temperature, would have been entirely driven off; and that, in theſe circumſtances, heat produces ſome of the identical effects aſcribed to it in the Huttonian Theory.

Pounded carbonate of lime in its ſeveral varieties became agglutinated into ſtony maſſes.

By this joint action of heat and preſſure, the carbonate of lime which had been introduced in the ſtate of the fineſt powder, is agglutinated into a firm maſs, poſſeſſing a degree of hardneſs, compactneſs, and ſpecific gravity* nearly approaching to theſe qualities in a ſound limeſtone; and ſome of the reſults, by their ſaline fracture, by their ſemitranſparency, and their ſuſceptibility of poliſh, deſerve the name of marble.

The ſame trials have been made with all calcareous ſubſtances; with chalk, common limeſtone, marble, ſpar, and the ſhells of fiſh. All have ſhewn the ſame general property, with ſome varieties as to temperature. Thus, I found, that, in the ſame circumſtances, chalk was more ſuſceptible of agglutination than ſpar; the latter requiring a heat two degrees higher than the former, to bring it to the ſame pitch of agglutination.

The chalk uſed in my firſt experiments, always aſſumed the character of a yellow marble, owing probably to ſome ſlight contamination of iron. When a ſolid piece of chalk, whoſe bulk had been previouſly meaſured in the gauge of Wedgwood's pyrometer was ſubmitted to heat under compreſſion, its contraction was remarkable, proving the approach of the particles during their conſolidation; on theſe occaſions, it was found to ſhrink three times more than the pyrometer-pieces in the ſame temperature. It loſt, too, almoſt entirely, its power of imbibing water, and acquired a great additional ſpecific gravity. On ſeveral occaſions, I obſerved, that maſſes of chalk, which, before the experiment, had ſhewn one uniform character of whiteneſs, aſſumed a ſtratified appearance, indicated by a ſeries of parallel layers of a brown colour. This circumſtance may hereafter throw light on the geological hiſtory of this extraordinary ſubſtance.

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* See Appendix.

And moſt of the carbonic acid was retained.

I have ſaid, that, by mechanical conſtraint, almoſt the whole of the carbonic acid was retained. And, in truth, at this period, ſome loſs of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumſtance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

Qualities of the product differing according to the loſs of carbonic acid.

When the loſs exceeded 10 or 15 *per cent.* * of the weight of the carbonate, the reſult was always of a friable texture, and without any ſtony character; when leſs than two or three *per cent.* it was conſidered as good, and poſſeſſed the properties of a natural carbonate. In the intermediate caſes, when the loſs amounted, for inſtance, to ſix or eight *per cent.* the reſult was ſometimes excellent at firſt, the ſubſtance bearing every appearance of ſoundneſs, and often poſſeſſing a high character of cryſtallization; but it was unable to reſiſt the action of the air: and, by attracting carbonic acid or moiſture, or both, crumbled to duſt more or leſs rapidly, according to circumſtances. This ſeems to prove, that the carbonate of lime, though not fully ſaturated with carbonic acid, may poſſeſs the properties of limeſtone; and perhaps a difference of this kind may exiſt among natural carbonates, and give riſe to their different degrees of durability.

I have obſerved, in many caſes, that the calcination has reached only to a certain depth into the maſs; the internal part remaining in a ſtate of complete carbonate, and, in general, of a very fine quality. The partial calcination ſeems thus to take place in two different modes. By one, a ſmall proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the reſt is left entire. Perhaps one reſult is the effect of a feeble calcining cauſe, acting during a long time, and the other of a ſtrong cauſe, acting for a ſhort time.

Some reſults were ſubject to decay from partial calcination.

Some of the reſults which ſeemed the moſt perfect when firſt produced, have been ſubject to decay, owing to partial calcination. It happened, in ſome degree, to the beautiful ſpecimen produced on the 3d of March, 1801, though a freſh fracture has reſtored it.

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* I have found, that, in open fire, the entire loſs ſuſtained by the carbonate varies in different kinds from 42 to 45.5 *per cent.*

A ſpecimen, too, of marble, formed from pounded ſpar, on the 15th of May, 1801, was ſo complete as to deceive the workman employed to poliſh it, who declared, that, were the ſubſtance a little whiter, the quarry from which it was taken would be of great value, if it lay within reach of a market. Yet, in a few weeks after its formation, it fell to duſt.

Very many were durable marble.

Numberleſs ſpecimens, however, have been obtained, which reſiſt the air, and retain their poliſh as well as any marble. Some of them continue in a perfect ſtate, though they have been kept without any precaution during four or five years. That ſet, in particular, remain perfectly entire, which were ſhewn laſt year in this Society, though ſome of them were made in 1799, ſome in 1801 and 1802, and though the firſt eleven were long ſoaked in water, in the trials made of their ſpecific gravity.

Remarkable fact.

A curious circumſtance occurred in one of theſe experiments, which may hereafter lead to important conſequences. Some ruſt of iron had accidentally found its way into the tube: 10 grains of carbonate were uſed, and a heat of 28° was applied. The tube had no flaw; but there was a certainty that the carbonic acid had eſcaped through its pores. When broken, the place of the carbonate was found occupied, partly by a black flaggy matter, and partly by ſphericles of various ſizes, from that of a ſmall pea downwards, of a white ſubſtance, which proved to be quicklime; the ſphericles being interſperſed through the flag, as ſpar and agates appear in whinſtone. The flag had certainly been produced by a mixture of the iron with the ſubſtance of the tube; and the ſpherical form of the quicklime ſeems to ſhew, that the carbonate had been in fuſion along with the flag, and that they had ſeparated on the eſcape of the carbonic acid.

The ſubject was carried thus far in 1803, when I ſhould probably have publiſhed my experiments, had I not been induced to proſecute the inquiry by certain indications, and accidental reſults, of a nature too irregular and uncertain to meet the public eye, but which convinced me, that it was poſſible to eſtabliſh by experiment the truth of all that was hypothetically aſſumed in the Huttonian theory.

Endeavour to improve the experiments by

The principal object was now to accompliſh the entire fuſion of the carbonate, and to obtain ſpar as the reſult of that fuſion, in imitation of what we conceive to have taken place in nature.

preventing all calcination, and perfectly fuſing the carbonate.

It was likewiſe important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itſelf, and on account of its conſequences; the reſult being viſibly improved by every approach towards complete ſaturation. I therefore became anxious to inveſtigate the cauſe of the partial calcinations which had always taken place, to a greater or leſs degree, in all theſe experiments. The queſtion naturally ſuggeſts itſelf, What has become of the carbonic acid, ſeparated in theſe partial calcinations from the earthy baſis? Has it penetrated the veſſel, and eſcaped entirely, or has it been retained within it in a gaſeous, but highly compreſſed ſtate? It occurred to me, that this queſtion might be eaſily reſolved, by weighing the veſſel before and after the action of heat upon the carbonate.

With iron, a conſtant and inappreciable ſource of irregularity exiſted in the oxidation of the barrel. But with porcelain the thing was eaſy; and I put it in practice in all my experiments with this material, which were made after the queſtion had occurred to me. The tube was weighed as ſoon as its muzzle was cloſed, and again, after the breech had been expoſed to the fire; taking care, in both caſes, to allow all to cool. In every caſe, I found ſome loſs of weight, proving, that even in the beſt experiments, the tubes were penetrated to a certain degree. I next wiſhed to try if any of the carbonic acid ſeparated, remained within the tube in a gaſeous form; and in that view, I wrapt the tube, which had juſt been weighed, in a ſheet of paper, and placed it, ſo ſurrounded, on the ſcale of the balance. As ſoon as its weight was aſcertained, I broke the tube by a ſmart blow, and then replaced upon the ſcale the paper containing all the fragments. In thoſe experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already eſcaped during the action of heat. But in the good reſults, I always found that a loſs of weight was the conſequence of breaking the tube.

By experiment it was found that iron barrels vitiated the reſult.

Theſe facts prove, that both cauſes of calcination had operated in the porcelain tubes; that, in the caſes of ſmall loſs, part of the carbonic acid had eſcaped through the veſſel, and that part had been retained within it. I had in view methods

With porcelain tubes this cauſe of irregularity exiſted along with the eſcape of the acid by which the laſt could be counteracted; but I ſaw no remedy for the firſt. I began, therefore, to deſpair of ultimate ſucceſs with tubes of porcelain *.

Theſe laſt could not bear elevated heats.

Another circumſtance confirmed me in this opinion. I found it impracticable to apply a heat above 27° to theſe tubes, when charged as above with carbonate, without deſtroying them, either by explosion, by the formation of a minute rent, or by the actual ſwelling of the tube. Sometimes this ſwelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate ſuſtaining but a very ſmall loſs. This ductility of the porcelain in a low heat is a curious fact, and ſhews what a range of temperature is embraced by the gradual tranſition of ſome ſubſtances from a ſolid to a liquid ſtate: For the ſame porcelain, which is thus ſuſceptible of being ſtretched out without breaking in a heat of 27°, ſtands the heat of 152°, without injury, when expoſed to no violence, the angles of its fracture remaining ſharp and entire.

IV.

Experiments in Gun-Barrels reſumed.—The Vertical Apparatus applied to them.—Barrels bored in ſolid Bars.—Old Sable Iron.—Fuſion of the Carbonate of Lime.—Its Action on Porcelain.—Additional Apparatus required in Conſequence of that Action.—Good Reſults; in particular, four Experiments, illuſtrating the Theory of Internal Calcination, *and ſhewing the Efficacy of the Carbonic Acid as a Flux.*

Experiments with gun barrels reſumed,

SINCE I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expoſe the carbonate in them to ſtrong heats; I at laſt determined to lay them aſide, and return to barrels of iron, with which I had formerly obtained ſome good reſults, favoured, perhaps, by ſome accidental circumſtances.

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* I am nevertheleſs of opinion, that, in ſome ſituations, experiments with compreſſion may be carried on with great eaſe and advantage in ſuch tubes. I allude to the ſituation of the geologiſts of France and Germany, who may eaſily procure, from their own manufactories, tubes of a quality far ſuperior to any thing made for ſale in this country.

On the 12th of February, 1803, I began a ſeries of experiments with gun-barrels, reſuming my former method of working with the fuſible metal, and with lead; but altering the poſition of the barrel from horizontal to vertical; the breech being placed upwards during the action of heat on the carbonate. This very ſimple improvement has been productive of advantages no leſs remarkable, than in the caſe of the tubes of porcelain. In this new poſition, the included air, quitting the air-tube on the fuſion of the metal, and riſing to the breech, is expoſed to the greateſt heat of the furnace, and muſt therefore react with its greateſt force; whereas, in the horizontal poſition, that air might go as far back as the fuſion of the metal reached, where its elaſticity would be much feebler. The ſame diſpoſition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a veſſel filled with water; which contributed very much both to the convenience and ſafety of theſe experiments.

In a vertical poſition, with the breech upwards, and ſtopped with fuſible metal.

In this view, making uſe of the brick-furnace with the vertical muffle, already deſcribed in page 384, I ordered a pit (*a a a fig.* 20.) to be excavated under it, for the purpoſe of receiving a water-veſſel. This veſſel (repreſented ſeparately, *fig.* 21.) was made of caſt iron; it was three inches in diameter, and three feet deep; and had a pipe (*d e*) ſtriking off from it at right angles, four or five inches below its rim, communicating with a cup (*e f*) at the diſtance of about two feet. The main veſſel being placed in the pit (*a a*) directly below the vertical muffle, and the cup ſtanding clear of the furnace, water poured into the cup flowed into the veſſel, and could thus conveniently be made to ſtand at any level. (The whole arrangement is repreſented in *fig.* 20.) The muzzle of the barrel (g) being plunged into the water, and its breech (*b*) reaching up into the muffle, as far as was found convenient, its poſition was ſecured by an iron chain (*g f*). The heat communicated downwards generally kept the ſurface of the water (at p) in a ſtate of ebullition; the waſte thus occaſioned being ſupplied by means of the cup, into which, if neceſſary, a conſtant ſtream could be made to flow.

Deſcription and drawing of the apparatus.

As formerly, I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel

would

would admit, in order to exclude as much of the fuſible metal as poſſible; for the expanſion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the leſs riſk there was of deſtroying the barrels.

Simple mode of withdrawing the contents from the tube.

In the courſe of practice, a ſimple mode occurred of removing the metal and withdrawing the cradle: it conſiſted in placing the barrel with its muzzle downwards, ſo as to keep the breech above the furnace and cold, while its muzzle was expoſed to ſtrong heat in the muffle. In this manner, the metal was diſcharged from the muzzle, and the poſition of the barrel being lowered by degrees, the whole metal was removed in ſucceſſion, till at laſt the cradle and its contents became entirely looſe. As the metal was delivered, it was received in a crucible, filled with water, ſtanding on a plate of iron placed over the pit, which had been uſed, during the firſt ſtage of the experiment, to contain the water-veſſel. It was found to be of ſervice, eſpecially where lead was uſed, to give much more heat to the muzzle than ſimply what was required to liquefy the metal it contained; for when this was not done, the muzzle growing cold as the breech was heating, ſome of the metal delivered from the breech was congealed at the muzzle, ſo as to ſtop the paſſage.

According to this method, many experiments were made in gun-barrels, by which ſome very material ſteps were gained in the inveſtigation.

Experiment in the new method.

On the 24th of February, I made an experiment with ſpar and chalk; the ſpar being placed neareſt to the breech of the barrel, and expoſed to the greateſt heat, ſome baked clay intervening between the carbonates. On opening the barrel, a long-continued hiſſing noiſe was heard. The ſpar was in a ſtate of entire calcination; the chalk, though crumbling at the outſide, was uncommonly hard and firm in the heart. The temperature had riſen to 32°.

Internal calci- ition, where the carbonic id did not ſcape out of he apparatus.

In this experiment, we have the firſt clear example, in iron barrels, of what I call *Internal Calcination*; that is to ſay, where the carbonic acid ſeparated from the earthy baſis, has been accumulated in cavities within the barrel. For, ſubſequently to the action of ſtrong heat, the barrel had been completely cooled; the air therefore introduced by means of the air-tube, muſt have reſumed its original bulk, and by itſelf could have no tendency to ruſh out; the heat employed to open the barrel [illegible] barely ſufficient to ſoften the metal. Since, the opening of the barrel was accompanied by the diſcharge of elaſtic matter in great abundance, it is evident, that this muſt have proceeded from ſomething ſuperadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at leaſt, internal; the ſeparation of the acid from the earthy matter being complete where the heat was ſtrongeſt, and only partial where the intenſity was leſs.

Part of the included carbonate was calcined, another part retaining its acid. Reaſoning on this fact.

The chemical principles ſtated in a former part of this paper, authoriſed us to expect a reſult of this kind. As heat, by increaſing the volatility of the acid, tended to ſeparate it from the earth, we had reaſon to expect, that, under the ſame compreſſion, but in different temperatures, one portion of the carbonate might be calcined, and another not: And that the leaſt heated of the two, would be the leaſt expoſed to a change not only from want of heat, but likewiſe in conſequence of the calcination of the other maſs; for the carbonic acid diſengaged by the calcination of the hotteſt of the two, muſt have added to the elaſticity of the confined elaſtic fluid, ſo as to produce an increaſe of compreſſion. By this means, the calcination of the coldeſt of the two might be altogether prevented, and that of the hotteſt might be hindered from making any further advancement. This reaſoning ſeemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened ſome new practical views in theſe experiments, of which I availed myſelf without loſs of time. If the internal calcination of one part of an incloſed maſs, promotes the compreſſion of other maſſes included along with it, I conceived that we might forward our views very much by placing a ſmall quantity of carbonate, carefully weighed, in the ſame barrel with a large quantity of that ſubſtance; and by arranging matters ſo that the ſmall fiducial part ſhould undergo a moderate heat, while a ſtronger heat, capable of producing internal calcination, ſhould be applied to the reſt of the carbonate. In this manner, I made many experiments, and obtained reſults which ſeemed to confirm this reaſoning, and which were often very ſatisfactory, though the heat did not always exert its greateſt force where I intended it to do ſo.

Experiment. Carbonate partly fuſed, and in part deprived of carbonic acid.

On the 28th of February, I introduced ſome carbonate, accurately weighed, into a ſmall porcelain tube, placed within a larger one, the reſt of the large tube being filled with pounded chalk; theſe carbonates, together with ſome pieces of chalk, placed along with the large tube in the cradle, weighing in all 195.7 grains. On opening the barrel, air ruſhed out with a long-continued hiſſing noiſe. The contents of the little tube were loſt by the intruſion of ſome borax which had been introduced over the ſilex, in order to exclude the fuſible metal. But the reſt of the carbonate, contained in the large tube, came out in a fine ſtate, being porous and frothy throughout; ſparkling every where with facetts, the angular form of which was diſtinguiſhable in ſome of the cavities by help of a lens: in ſome parts the ſubſtance exhibited the rounding of fuſion; in many it was in a high degree tranſparent. It was yellow towards the lower end, and at the other almoſt colourleſs. At the upper end, the carbonate ſeemed to have united with the tube, and at the places of contact to have ſpread upon it; the union having the appearance of a mutual action. The general maſs of carbonate effervesced in acid violently, but the thin ſtratum immediately contiguous to the tube, feebly, if at all.

Similar experiment, in which the carbonate exhibited more remarkable facettes.

On the 3d of March, I introduced into a very clean tube of porcelain 30.8 of chalk. The tube was placed in the upper part of the cradle, the remaining ſpace being filled with two pieces of chalk, cut for the purpoſe; the uppermoſt of theſe being excavated, ſo as to anſwer the purpoſe of an air-tube. The pieces thus added, were computed to weigh about 300 grains. There was no pyrometer uſed; but the heat was gueſſed to be about 30°. After the barrel had ſtood during a few minutes in its delivering poſition, the whole lead with the rod and cradle, were thrown out with a ſmart report, and with conſiderable force. The lowermoſt piece of chalk had ſcarcely been acted upon by heat. The upper part of the other piece was in a ſtate of marble, with ſome remarkable facettes. The carbonate, in the little tube, had ſhrunk very much during the firſt action of heat, and had begun to ſink upon itſelf, by a further advancement towards liquefaction. The maſs was divided into ſeveral cylinders, lying confuſedly upon each other; this diviſion ariſing from the manner in which the pounded chalk was rammed into the tube in ſucceſſive portions. In ſeveral places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a ſoft ſubſtance; the globular form and ſhining ſurface of all theſe cavities, clearly indicating fuſion. The ſubſtance was ſemi-tranſparent; in ſome places yellow, and in ſome colourleſs. When broken, the ſolid parts ſhewed a ſaline fracture, compoſed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, ſo as to render it impoſſible to aſcertain what loſs had been ſuſtained. In general, the line of contact was of a brown colour; yet there was no room for ſuſpecting the preſence of any foreign matter, except, perhaps, from the iron-rod which was uſed in ramming down the chalk. But, in ſubſequent experiments, I have obſerved the ſame brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purpoſely rammed with a piece of wood; ſo that this colour, which has occurred in almoſt every ſimilar caſe, remains to be accounted for. The carbonate efferveſced violently with acid; the ſubſtance in contact with the tube, doing ſo, however, more feebly than in the heart, leaving a copious depoſit of white ſandy matter, which is doubtleſs a part of the tube, taken up by the carbonate in fuſion.

Another experiment with ſlow cooling.

Saline ſtructure and cryſtallization produced in chalk previouſly pounded.

On the 24th of March, I made a ſimilar experiment, in a ſtout gun-barrel, and took ſome care, after the application of heat, to cool the barrel ſlowly, with a view to cryſtallization. The whole maſs was found in a fine ſtate, and untouched by the lead;* having a ſemitranſparent and ſaline ſtructure, with various facettes. In one part, I found the moſt decided cryſtallization I had obtained, though of a ſmall ſize: owing to its tranſparency it was not eaſily viſible, till the light was made to reflect from the cryſtalline ſurface, which then produced a dazzle, very obſervable by the naked eye; when examined by means of a lens, it was ſeen to be compoſed of ſeveral plates, broken irregularly in the fracture of the ſpecimen, all of which are parallel to each other, and reflect under the ſame angle ſo as to unite in producing the dazzle. This ſtructure was obſervable equally well in both parts of the broken ſpecimen. In a former experiment, as large a facette was obtained in a piece of ſolid chalk; but this reſult was of more conſequence, as having been produced from chalk previouſly pounded.

The gun-barrels, though ſuperior to porcelain were ſtill too weak.

The foregoing experiments proved the ſuperior efficacy of iron veſſels over thoſe of porcelain, even where the thickneſs was not great; and I perſevered in making a great many experiments with gun-barrels, by which I occaſionally obtained very fine reſults: but I was at laſt convinced, that their thickneſs was not ſufficient to enſure regular and ſteady ſucceſs. For this purpoſe, it appeared proper to employ veſſels of ſuch ſtrength, as to bear a greater expanſive force than was juſt neceſſary; ſince, occaſionally, (owing to our ignorance of the relation between the various forces of expanſion, affinity, tenacity, &c.) much more ſtrain has been given to the veſſels than was requiſite. In ſuch caſes, barrels have been deſtroyed, which, as the reſults have proved, had acted with ſufficient ſtrength during the firſt ſtages of the experiments, though they had been unable to reſiſt the ſubſequent overſtrain. Thus, my ſucceſs with gun-barrels, depended on the good fortune of having uſed a force no more than ſufficient, to conſtrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourſe to iron barrels of much greater ſtrength, and tried various modes of conſtruction.

Barrels formed by boring in ſolid bars of iron which proved excellent.

I had ſome barrels executed by wrapping a thick plate of iron round a mandrel, as is practiſed in the formation of gun-barrels; and likewiſe by bringing the two flat ſides together, ſo as to unite them by welding. Theſe attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the ſolid, ſo as to form a barrel. In this manner I ſucceeded well. The firſt barrel I tried in this way was of ſmall bore, only half an inch: Its performance was highly ſatisfactory, and ſuch as to convince me, that the mode now adopted was the beſt of any that I had tried. Owing to the ſmallneſs of the bore, a pyrometer could not be uſed internally, but was placed upon the breech of the barrel, as it ſtood in the vertical muffle. In this poſition, it was evidently expoſed to a much leſs heat than the fiducial part of the apparatus, which was always placed, as nearly as could be gueſſed, at the point of greateſt heat.

Finely levigated ſpar became agglutinated by heat; ſemi-tranſparent, vitreous, with a few facettes.

On the 4th of April, an experiment was made in this way with ſome ſpar; the pyrometer on the breech giving 33°. The ſpar came out clean, and free from any contamination, adhering to the inſide of the porcelain tube: it was very much ſhrunk, ſtill retaining a cylindrical form, though bent by partial adheſions. Its ſurface bore ſcarcely any remains of the impreſſion taken by the powder, on ramming it into the tube: it had, to the naked eye, the roughneſs and ſemitranſparency of the pith of a ruſh ſtripped of its outer ſkin. By the lens, this ſame ſurface was ſeen to be glazed all over, though irregularly, ſhewing here and there ſome air-holes. In fracture, it was ſemitranſparent, more vitreous than cryſtalline, though having a few facettes: the maſs, was ſeemingly formed of a congeries of parts, in themſelves quite tranſparent: and, at the thin edges, ſmall pieces were viſible of perfect tranſparency. Theſe muſt have been produced in the fire; for the ſpar had been ground with water; and paſſed through ſieves, the ſame with the fineſt of thoſe uſed at Etruria, as deſcribed by Mr. Wedgwood, in his paper on the conſtruction of his pyrometer.

With the ſame barrel I obtained many intereſting reſults, giving as ſtrong proofs of fuſion as in any former experiments; with this remarkable difference, that, in theſe laſt, the ſubſtance was compact, with little or no trace of frothing. In the gun-barrels where fuſion had taken place, there had always been a loſs of 4 or 5 *per cent.* connected, probably, with the frothing. In theſe experiments, for a reaſon ſoon to be ſtated, the circumſtance of weight could'not be obſerved; but appearances led me to ſuppoſe, that here the loſs had been ſmall, if any.

In theſe experiments the eſcape of carbonic acid appears to have been leſs.

On the 6th of April, I made another experiment with the ſquare barrel, whoſe thickneſs was now much reduced by ſucceſſive ſcales, produced by oxidation, and in which a ſmall rent began to appear externally, which did not, however, penetrate to the bore. The heat roſe high, a pyrometer on the breech of the barrel giving 37°. On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident, that the barrel had not been laid open, for part of the carbonate was in a ſtate of ſaline marble; another was hard and white, without any ſaline grains, and ſcarcely efferveſced in acid. It was probably quicklime, formed by internal calcination, but in a ſtate that has not occurred in any other experiment.

Gradual failure of the barrel bored from the ſolid.

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Remarkable fact of cryſtals which appear to have been formed by ſublimation

The workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was ſeen in this piece, I was ſurpriſed to ſee the inſide of the barrel lined with a ſet of tranſparent and well-defined cryſtals, of ſmall ſize, yet viſible by the naked eye. They lay together in ſome places, ſo as to cover the ſurface of the iron with a tranſparent coat; in others they were detached, and ſcattered over the ſurface. Unfortunately, the quantity of this ſubſtance was too ſmall to admit of much chemical examination; but I immediately aſcertained, that it did not in the leaſt efferveſce in acid, nor did it ſeem to diſſolve in it. The cryſtals were in general tranſparent and colourleſs, though a few of them were tinged ſeemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a ſtrong reſemblance to the cryſtals of the Lamellated Stylbite of Hauy. Though made above two years ago, they ſtill retain their form and tranſparency unchanged. Whatever this ſubſtance may be, its appearance, in this experiment, is in the higheſt degree intereſting, as it ſeems to afford an example of the mode in which Dr. Hutton ſuppoſes many internal cavities to have been lined, by the ſublimation of ſubſtances in a ſtate of vapour; or, held in ſolution, by matters in a gaſeous form. For, as the cryſtals adhered to a part of the barrel, which muſt have been occupied by air during the action of heat, it ſeems next to certain that they were produced by ſublimation.

The old Sable Siberian iron is very tough at high heats.

The very powerful effects produced by this laſt barrel, the ſize of which (reduced, indeed, by repeated oxidation) was not above an inch ſquare, made me very anxious to obtain barrels of the ſame ſubſtance, which being made of greater ſize, ought to afford reſults of extreme intereſt. I found upon inquiry, that this barrel was not made of Swediſh iron, as I at firſt ſuppoſed, but of what is known by the name of *Old Sable*, from the figure of a Sable ſtamped upon the bars; that being the armorial badge of the place in Siberia where this iron is made.*

All iron is cruſhed under the hammer at ſome definite heat. Caſt iron at a low heat;

A workman explained to me ſome of the properties of different kinds of irons, moſt intereſting in my preſent purſuit; and he illuſtrated what he ſaid by actual trial. All iron, when expoſed to a certain heat, cruſhes and crumbles under the hammer; but the temperature in which this happens, varies with every different ſpecies. Thus, as he ſhewed me, caſt iron cruſhes in a dull red heat, or perhaps about 15° of Wedgwood; ſteel, in a heat perhaps of 30°; Swediſh iron, in a bright white heat, perhaps of 50° or 60°; old ſable itſelf, likewiſe yields, but in a much higher heat, perhaps of 100°. I merely gueſſed at theſe temperatures; but I am certain of this, that in a heat ſimilar to that in which Swediſh iron crumbled under the hammer, the old ſable withſtood a ſtrong blow, and ſeemed to poſſeſs conſiderable firmneſs. It is from a knowledge of this quality, that the blackſmith, when he firſt takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has ſunk to the degree in which the iron can bear the hammer. I obſerved, as the ſtrong heat of the forge acted on the Swediſh iron, that it began to boil at the ſurface, clearly indicating the diſcharge of ſome gaſeous matter; whereas, the old ſable, in the ſame circumſtances, acquired the ſhining ſurface of a liquid, and melted away without any efferveſcence. I procured, at this time, a conſiderable number of bars of that iron, which fully anſwered my expectations.

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* I was favoured with this account by the late Profeſſor Robiſon.

ſteel at a higher, Swediſh iron at a bright white heat, and old Sable at a ſtill higher temperature.

By the experiments laſt mentioned, a very important point was gained in this inveſtigation; the complete fuſibility of the carbonate under preſſure being thereby eſtabliſhed. But from this very circumſtance, a neceſſity aroſe of adding ſome new devices to thoſe already deſcribed: for the carbonate, in fuſion, ſpreading itſelf on the inſide of the tube containing it, and the two uniting firmly together, ſo as to be quite inſeparable, it was impoſſible, after the experiment, to aſcertain the weight of the carbonate by any method previouſly uſed. I therefore determined in future to adopt the following arrangement.

The complete fuſibility of the carbonate under preſſure was aſcertained in theſe ſtrong barrels.

A ſmall tube of porcelain (*ik*, *Fig.* 23.) was weighed by means of a counterpoiſe of ſand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment was aſcertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had

Arrangement for obviating ſome difficulties which aroſe from the fuſion. The carbonate was put into a ſmall porcelain tube and this properly ſecured in a larger. Theſe

taken

were placed in a cradle or frame and the whole put into the iron barrel, &c.

taken place between the tube and the carbonate. The balance which I uſed, turned in a conſtant and ſteady manner, with one hundredth of a grain. When pounded chalk was rammed into this tube, I generally left part of it free, and in that ſpace laid a ſmall piece of lump-chalk (*i*), dreſſed to a cylinder, with the ends cut flat and ſmooth, and I uſually cut a letter on each end, the more effectually to obſerve the effects produced by heat upon the chalk; the weight of this piece of chalk being always eſtimated along with that of the powder contained in the tube. In ſome experiments, I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it), in order to provide againſt the caſe of ebullition: but as that did not often occur, I ſeldom took the trouble of this laſt precaution.

Continuation of the method of experiment.

It was now of conſequence to protect the tube, thus prepared, from being touched during the experiment, by any ſubſtance, above all, by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The ſmall tube (*Fig.* 23, *i k*) with its pounded carbonate (*k*), and its cylinder of lump-chalk (*i*), was dropt into a large tube of porcelain (*p k*, *Fig.* 24). Upon this a fragment of porcelain (*l*), of ſuch a ſize as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dreſſed, ſo as nearly to fit and fill up the inſide of the large tube, one end of it being rudely cut into the form of a cone. This maſs being then introduced, with its cylindrical end downwards, was made to preſs upon the fragment of porcelain (*l*). I then dropped into the ſpace (*n*), between the conical part of this maſs and the tube, a ſet of fragments of chalk, of a ſize beyond what could poſſibly fall between the cylindrical part and the tube, and preſſed them down with a blunt tool, by which the chalk being at the ſame time cruſhed and rammed into the angle, was forced into a maſs of ſome ſolidity, which effectually prevented any thing from paſſing between the large maſs of chalk and the tube. In practice, I have found this method always to anſwer, when done with care. I covered the chalk, thus rammed, with a ſtratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner, I filled the whole of the large tube with alternate layers of ſilex and chalk; the muzzle being always occupied with chalk, which was eaſily preſſed into a maſs of tolerable

firmneſs

Continuation of the method of experiment.

firmneſs, and, ſuffering no change in very low heats, excluded the fuſible metal in the firſt ſtages of the experiment.

The large tube, thus filled, was placed in the cradle, ſometimes with the muzzle upwards, and ſometimes the reverſe. I have frequently altered my views as to that part of the arrangement, each mode poſſeſſing peculiar advantages and diſadvantages. With the muzzle upwards, (as ſhewn in *Fig.* 24 and 25), the beſt ſecurity is afforded againſt the intruſion of the fuſible metal; becauſe the air, quitting the air-tube in the working poſition, occupies the upper part of the barrel; and the fuſible metal ſtands as a liquid (at *q*, *Fig.* 25.) below the muzzle of the tube, ſo that all communication is cut off, between the liquid metal and the inſide of the tube. On the other hand, by this arrangement, the ſmall tube, which is the fiducial part of the apparatus, is placed at a conſiderable diſtance from the breech of the barrel, ſo as either to undergo leſs heat than the upper part, or to render it neceſſary that the barrel be thruſt high into the muffle.

With the muzzle of the large tube downwards, the inner tube is placed (as ſhewn in *Fig.* 22), ſo as ſtill to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the ſmall tube near to the breech of the barrel: and though there is here leſs ſecurity againſt the intruſion of liquid metal, I have found that a point of little conſequence; ſince, when the experiment is a good one, and that the carbonic acid has been well confined, the intruſion ſeldom takes place in any poſition. In whichever of the two oppoſite poſitions the large tube was placed, a pyrometer was always introduced, ſo as to lie as near as poſſible to the ſmall tube. Thus, in the firſt-mentioned poſition, the pyrometer was placed immediately below the large tube, and, in the other poſition above it; ſo that, in both caſes, it was ſeparated from the carbonate by the thickneſs only of the two tubes.

Much room was unavoidably occupied by this method, which neceſſarily obliged me to uſe ſmall quantities of carbonate, the ſubject of experiment ſeldom weighing more than 10 or 12 grains, and in others far leſs*.

On

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* I meaſured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal ſand. By comparing the weight of

Experiment made with the foregoing precautions. The heat amounted to 64° Wedgwood. The carbonates had loſt gas and undergone fuſion.

On the 11th of April, 1803, with a barrel of old ſable iron having a bore of 0.75 of an inch, I made an experiment in which all theſe arrangements were put in practice. The large tube contained two ſmall ones; one filled with ſpar, and the other with chalk. I conceived that the heat had riſen to 33°, or ſomewhat higher. On melting the metals, the cradle was thrown out with conſiderable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my aſtoniſhment, indicated 64°. Yet all was ſound. The two little tubes came out quite clean and uncontaminated. The ſpar had loſt 17.0 *per cent.* the chalk 10.7 *per cent.* The ſpar was half ſunk down, and run againſt the ſide of the little tube: Its ſurface was ſhining, its texture ſpongy, and it was compoſed of a tranſparent and jelly-like ſubſtance. The chalk was entirely in a ſtate of froth. This experiment extends our power of action, by ſhewing, that compreſſion, to a conſiderable degree, can be carried on in ſo great a heat as 64°. It ſeems likewiſe to prove, that, in ſome of the late experiments with the ſquare barrel, the heat had been much higher than was ſuppoſed at the time, from the indication of the pyrometer placed on the breech of the barrel; and that in ſome of them, particularly in the laſt, it muſt have riſen at leaſt as high as in the preſent experiment.

Experiment in which the barrel failed after its contents had undergone fuſion.

On the 21ſt of April, 1805, a ſimilar experiment was made with a new barrel, bored in a ſquare bar of old ſable, of about two and a half inch in diameter, having its angles merely rounded; the inner tube being filled with chalk. The heat was maintained during ſeveral hours, and the furnace allowed to burn out during the night. The barrel had the appearance of ſoundneſs, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63°. On examination, and after beating off the ſmooth and even ſcale of oxide peculiar to the old ſable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithſtanding the failure of the barrel, was one of the moſt intereſting I had

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of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330.6 grains, and that each grain of it correſponded to 0.00075 of a cubic inch. From theſe data I was able, with tolerable accuracy, to gage a tube by weighing the tin required to fill it.

made, ſince it afforded proof of complete fuſion. The carbonate had boiled over the lips of the little tube, ſtanding, as juſt deſcribed, with its mouth upwards, and had run down to within half an inch of its lower end: moſt of the ſubſtance was in a frothy ſtate, with large round cavities, and a ſhining ſurface; in other parts, it was interſperſed with angular maſſes, which have evidently been ſurrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no efferveſcence, and not turning red like quicklime in nitric acid, which ſeemed to have no effect upon it in the lump. It was probably a compound of quicklime with the ſubſtance of the tube.

With the ſame barrel repaired, and with others like it, many ſimilar experiments were made at this time with great ſucceſs; but to mention them in detail, would amount nearly to a repetition of what has been ſaid. I ſhall take notice of only four of them, which, when compared together, throw much light on the theory of theſe operations, and likewiſe ſeem to eſtabliſh a very important principle in geology. Theſe four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

Account of ſome experiments at very elevated heats. The carbonate moſt heated was calcined; that which had ſuffered leſs heat had the form of lime-ſtone and of marble, which retained their carbonic acid.

The firſt of theſe experiments was made on the 27th of April 1803, in one of the large barrels of old ſable, with all the above-mentioned arrangements. The heat had riſen, contrary to my intention, to 78° and 79°. The tubes came out uncontaminated with fuſible metal, and every thing bore the appearance of ſoundneſs. The contents of the little tube, conſiſting of pounded chalk, and of a ſmall piece of lump-chalk, came out clean, and quite looſe, not having adhered to the inſide of the tube in the ſmalleſt degree. There was a loſs of 41 *per cent.* and the calcination ſeemed to be complete; the ſubſtance, when thrown into nitric acid, turning red, without efferveſcence at firſt, though, after lying a few minutes, ſome bubbles appeared. According to the method followed in all theſe experiments, and lately deſcribed at length, (and ſhewn in *Fig.* 24 and 25), the large tube was filled over the ſmall one, with various maſſes of chalk, ſome in lump, and ſome rammed into it in powder; and in the cradle there lay ſome pieces of chalk, filling up the ſpace, ſo that in the cradle there was a continued chain of carbonate of four or five inches in length. The ſubſtance was found to be leſs and leſs calcined, the more it was removed from the breech of the barrel, where the heat was greateſt. A ſmall piece of chalk, placed at the diſtance of half an inch from the ſmall tube, had ſome ſaline ſubſtance in the heart, ſurrounded and intermixed with quicklime, diſtinguiſhed by its dull white. In nitric acid, this ſubſtance became red, but efferveſced pretty briſkly; the efferveſcence continuing till the whole was diſſolved. The next portion of chalk was in a firm ſtate of limeſtone; and a lump of chalk in the cradle, was equal in perfection to any marble I have obtained by compreſſion: the two laſt-mentioned pieces of chalk efferveſcing with violence in the acid, and ſhewing no redneſs when thrown into it. Theſe facts clearly prove, that the calcination of the contents of the ſmall tube had been internal, owing to the violent heat which had ſeparated its acid from the moſt heated part of the carbonate, according to the theory already ſtated. The ſoundneſs of the barrel was proved by the complete ſtate of thoſe carbonates which lay in leſs heated parts. The air-tube in this experiment had a capacity of 0.29, nearly one-third of a cubic inch.

Another experiment in which the barrel failed.

The ſecond of theſe experiments was made on the 29th of April, in the ſame barrel with the laſt, after it had afforded ſome good reſults. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch.—The heat roſe to 60°. The barrel was covered externally with a black ſpongy ſubſtance, the conſtant indication of failure, and a ſmall drop of white metal made its appearance. The cradle was removed without any exploſion or hiſſing. The carbonates were entirely calcined. The barrel had yielded, but had reſiſted well at firſt; for the contents of the little tube were found in a complete ſtate of froth, and running with the porcelain.

Third experiment, very thin fuſion.

The third experiment was made on the 30th of April, in another ſimilar barrel. Every circumſtance was the ſame as in the two laſt experiments, only that the air-tube was now reduced to half its laſt bulk, that is, to one-twentieth of a cubic inch: A pyrometer was placed at each end of the large tube. The uppermoſt gave 41°, the other only 15°. The contents of the inner tube had loſt 16 *per cent.* and were reduced to a moſt beautiful ſtate of froth, not very much injured by the internal calcination and indicating a thinner ſtate of fuſion than had appeared.

The

Fourth experiment under greater preſſure, fuſion at a moderate heat, ſlight loſs of carbonic acid, &c.

The fourth experiment was made on the 2d of May, like the reſt in all reſpects, with a ſtill ſmaller air-tube, of 0.0318, being leſs than one thirtieth of a cubic inch. The upper pyrometer gave 25°, and the under one 16°: The loweſt maſſes of carbonate were ſcarcely affected by the heat. The contents of the little tube loſt 2.9 *per cent.* both the lump and the pounded chalk were in a fine ſaline ſtate, and, in ſeveral places, had run and ſpread upon the inſide of the tube, which I had not expected to ſee in ſuch a low heat. On the upper ſurface of the chalk rammed into the little tube, which, after its introduction had been wiped ſmooth, were a ſet of white cryſtals, with ſhining facettes, large enough to be diſtinguiſhed by the naked eye, and ſeeming to riſe out of the maſs of carbonate. I likewiſe obſerved, that the ſolid maſs on which theſe cryſtals ſtood, was uncommonly tranſparent.

Obſervations: the fuſion takes place at lower heats when the eſcape of carbonic acid is prevented. The acid acts as a flux.

In theſe four experiments, the bulk of the included air was ſucceſſively diminiſhed, and by that means its elaſticity increaſed. The conſequence was, that in the firſt experiment, where that elaſticity was the leaſt, the carbonic acid was allowed to ſeparate from the lime, in an early ſtage of the riſing heat, lower than the fuſing point of the carbonate, and complete internal calcination was effected. In the ſecond experiment, the elaſtic force being much greater, calcination was prevented, till the heat roſe ſo high as to occaſion the entire fuſion of the carbonate, and its action on the tube, before the carbonic acid was ſet at liberty by the failure of the barrel. In the third experiment, with ſtill greater elaſtic force, the carbonate was partly calcined, and its fuſion accompliſhed, in a heat between 41° and 15°. In the laſt experiment, where the force was ſtrongeſt of all, the carbonate was almoſt completely protected from decompoſition by heat, in conſequence of which it cryſtallized and acted on the tube, in a temperature between 25° and 16°. On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced; ſince the firſt experiment proved that quicklime by itſelf could neither be melted, nor act upon porcelain, even in the violent heat of 79°; whereas, in the laſt experiment where the carbonic acid was retained, both of theſe effects took place in a very low temperature.

(*To be continued.*)

IV.

Obſervations on the Effect of Madder Root on the Bones of Animals. By Mr. B. GIBSON.*

Account of the firſt diſcovery of the property of madder to tinge the bones of living animals.

THERE is, perhaps, no phenomenon, which occurs in an animal body more curious, than the tinge communicated to the bones of living animals, whoſe food has been mixed with madder root. This, like many other facts, to which no reaſoning *à priori* could have directed us, was diſcovered by chance. Mr. Belcher, dining with a calico printer on a leg of freſh pork, was ſurprized that the bones, inſtead of poſſeſſing their uſual whiteneſs, were of a deep red colour; and on enquiring the cauſe of it, was informed, that the pig had been fed upon the refuſe of the dyers' vats, and had received ſo much of the colouring matter of madder into the ſyſtem, that its bones were dyed by it. So intereſting a fact has attracted very much the attention of anatomiſts, and has been uſed in many phyſiological and pathological enquiries; it may not therefore be unintereſting to give a ſhort hiſtory of the phenomena connected with it, and the purpoſes to which it has been applied, previous to entering upon the more immediate object of this paper.

Experiments ſhewing that the tinge is more quickly given to the bones of growing animals.

Many experiments have been made to aſcertain how long a time is required to produce the tinge, and whether it be permanent or only temporary. Belcher and Morand, about the ſame time, mixed madder root with the food of chickens and young pigeons. The reſult of their obſervations was, that the tinge was more quickly communicated to the bones of growing animals, than to the bones of animals which had already completed their growth; the bones of young pigeons being tinged of a roſe-colour in twenty-four hours, and of a deep ſcarlet in three days; whilſt the bones of adult animals only exhibited a roſe-colour in fifteen days. They found the tinge moſt intenſe in the ſolid parts of thoſe bones, which were neareſt to the centre of circulation; whilſt in bones of equal ſolidity, at a greater diſtance from the heart, the tint was more faint. The dye was deep in proportion to the length of time the madder had been continued, and when it was diſcontinued, the colour gradually became more and more faint, till

Short time and other facts.

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* Mancheſter Memoirs, 1805.

it entirely diſappeared. According to the experiments of theſe gentlemen, other vegetable dyes, ſuch as logwood, turmeric and alkanet root, did not communicate their reſpective tints to the bones.*

Du Hamel uſed this property to ſhew the growth of bones.

This effect of madder upon the bones, was ſoon afterwards made uſe of by Du Hamel, in his attempt to prove the manner in which the bones of animals are encreaſed in thickneſs. Obſerving in the vegetable kingdom, that the bark, by a ſort of ſecretion, formed the ligneous part of a tree, in ſucceſſive layers; ſo he conceived that the perioſteum, or membrane ſurrounding bones, being converted into oſſeous matter, encreaſed their diameter by adding to them concentric laminæ in ſucceſſion. In order to prove the juſtneſs of his opinion, he mixed the food of a cock with madder root for a month, withheld it for a month, and then gave it again. He afterwards killed the animal, and upon inſpection thought he obſerved the appearance which he expected; viz. two layers of red bone incloſing one of white, correſponding to the periods of the madder's being given or withheld.

It is very doubtful whether that growth could be ſo indicated.

This experiment, and ſome others related by Du Hamel, appear to be concluſive in favour of the theory, which he wiſhed to eſtabliſh; and as they were conducted by a phyſiologiſt of high character, the accuracy of the obſervations could not have been doubted, had theſe experiments ſtood alone. But when they are compared with ſome of his own previous experiments, and thoſe of other authors, it is difficult to reconcile them. In ſome of Du Hamel's experiments, for inſtance, the bones of a cock were tinged of a roſe-colour through their whole ſubſtance in ſixteen days, and thoſe of young pigeons of a deep ſcarlet in three days. In ſeveral experiments I have made on the ſubject, I have found the bones of young pigeons tinged of a uniform roſe-colour, internally as well as externally, in twenty-four hours. This communication of colour to the whole ſubſtance of the oſſeous ſyſtem in ſo ſhort a time, makes it highly improbable that the laminated appearance, remarked by Du Hamel, was produced by the new formation of red and white oſſeous layers, correſponding to the times (months) the madder had been given or withheld. For, as Mr. John Bell very juſtly remarks,* "If a bone ſhould increaſe by layers thick enough to be viſible and of a diſtinct tint, and ſuch layers be continually accumulated upon each other every week, what kind of bone ſhould this grow to?" The only way in which we can reconcile with each other the phenomena obſerved in the different experiments, and account for their apparent contradiction, is, by ſuppoſing that Du Hamel miſtook for an obſcurely laminated appearance, the variety in the tint, which is more deeply communicated to the more ſolid, and more faintly to the leſs compact parts of a bone.

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* From ſome experiments I made on young pigeons, I found that a conſiderable quantity of logwood, in the form of extract, communicated an evidently purple tint to the bones. With regard to turmeric, it appears to be altered in its colour by paſſing through the digeſtive organs, for the fœces of the animals, who took it in conſiderable quantity, were conſtantly green: whilſt either logwood or madder root exhibited their reſpective hues after paſſing through the inteſtines. Saffron exhibits properties different from any of theſe ſubſtances; for though a pigeon took it in conſiderable quantity, and thereby had its fœces tinged, yet no perceptible alteration of colour was produced in its bones.

Late experiments of Dr. M'Donald on the bones.

This property of madder of tinging the bones of animals, has lately been employed by Dr. M'Donald,† in his ingenious reſearches into the formation and death of bones. Amongſt other objects, he attempted to aſcertain in what manner and how ſoon a cylindrical bone is regenerated to ſupply the place of one artificially killed. As the proceſs is highly curious, I ſhall briefly relate the principal points.

Very curious proceſs of a bone deſtroyed,

Dr. M'Donald's experiments were made by amputating the proper leg-bone of young pigeons or chickens immediately above the joint. The marrow was then extracted, and the cavity which contained it, filled with lint. This proceſs cauſed the death of the bone, and the formation of a new bone ſurrounding that deſtroyed enſued. Immediately after the experiment, the animal had its food mixed with madder root, and the part was inſpected in different animals, at different periods.

—and the regular proceſs of

On examination three days afterwards, the perioſteum or enveloping membrane, was found much thickened; and underneath it a gelatinous humour was effuſed, ſurrounding the dead bone, and ſpotted with red oſſeous nuclei; proving that the regeneration of the bone had commenced at this early period.

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* Anatomy of the bones, &c. p. 15.

† Diſputatio inauguralis de Necroſi ac Callo. 1799.

—its regeneration.

In ſeven days the new bone was found ſoft and flexible, not to be diſtinguiſhed from cartilage or griſtle, except by the red tint the madder had communicated to it; yet the bone deſtroyed was not at all coloured, although the other bones of the animal had acquired a bright red. From this time the new bone continued to encreaſe in hardneſs, ſurrounding the old one like a ſheath. The latter in about three weeks was ſo looſe as to be drawn out, and in about fifteen days from this time, the cavity of the regenerated bone was filled with marrow, and in every reſpect performed the office of that for which it was a ſubſtitute. This may be conſidered as a general outline of the progreſſive changes which take place during the regeneration of a cylindrical bone, in a young animal, ſuch as a pigeon, or chicken; and the ſame proceſs is frequently performed in the human body, when, from ſome internal cauſe, the life of a bone is deſtroyed. Theſe changes involve many intereſting particulars; but the circumſtance moſt immediately connected with the ſubject of this paper is, that although the ſhaft of the bone required three weeks for its renewal, yet in ſeven days the oſſeous ſyſtem generally had acquired a bright red. Now if we explain this change in colour according to the common opinion of abſorption of the white, and depoſition of the red oſſeous matter,* we muſt neceſſarily draw this concluſion; that the oſſeous ſyſtem of the animal will be renewed three times during the period, which the formation of the ſubſtitute bone requires; a concluſion which we ſhould be inclined to reject, merely from its improbability. But beſides this, the appear-

Inference. From the very ſpeedy acquiſition and ſubſequent loſs of the red colour that the oſſeous ſyſtem was naturally abſorbed and renewed in that period.

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* The common opinion of phyſiologiſts, with regard to this curious fact, is, that when a bone becomes red, during the exhibition of madder root, the white oſſeous particles which compoſed it, have been entirely removed by abſorption and replaced by new oſſeous matter of a red colour: and when a bone aſſumes its natural colour, theſe red particles have been removed and replaced by white. If this be the fact, it neceſſarily follows, that an animal has at leaſt fifty-two new ſets of bones in a year; for the oſſeous ſyſtem, according to the experiments of the moſt reſpectable phyſiologiſts, acquires a deep red tint from madder in one week, and aſſumes its natural colour in another.

ance of the parts ſtrongly militate againſt it—for, if we may judge at all of the activity of the proceſs in the two parts, by their comparative degrees of vaſcularity, that employed in forming the ſubſtitute bone far exceeds that going on in the oſſeous ſyſtem generally; one ſtriking phenomenon attending the regeneration of a bone being, the very high degree of increaſed vaſcularity which the parts employed in the proceſs rapidly aſſume.

Cauſe of doubt.

The bones are alone reddened by madder, becauſe the phoſphate of lime acts as a mordent on the madder.

After this effect of madder upon the bones was known, it long remained a myſtery, why ſome other white parts of the body, ſuch as nerves, cartilages and perioſteum, were not equally liable to be coloured by it, as the bones. This fact, I believe, did not receive any explanation, until Dr. Rutherford gave a very ingenious and ſatisfactory one. When ſpeaking of this property of madder, he ſays,* "We have, in the fact before us, a beautiful example of a particular caſe of chemical attraction; ſuch as in numberleſs inſtances, is obſerved to take place between the colouring particles of both animal and vegetable ſubſtances and various other bodies, eſpecially earths and earthy ſalts, and oxides of metals. So ſtrong is the affinity of the colouring matter to theſe bodies, that it is frequently obſerved to quit the menſtruum, in which it may chance to be diſſolved, to unite with them: they, in conſequence of its union, acquiring a particular tinge, whilſt the menſtruum is proportionably deprived of colour. From this principle, this mutual attraction, is deduced the various uſe of thoſe bodies as mordents, as they are called, intermedia, or means for fixing the colours in dying or ſtaining thread or cloth, whether it be compoſed of animal or vegetable materials. Upon the ſame principle depends the preparation of thoſe pigments known to painters under the name of lakes; theſe are truly precipitates of the colouring matter, in combination with various mordents, as their baſis.—The colouring of the bones of a living animal by means of madder, is, in every circumſtance, analogous to the formation of theſe lakes. The colouring matter of madder, paſſing unaltered through the digeſtive organs of the animal, enters the general maſs of fluids, and is diſſolved in the ſerum of the blood, to which,

The red matter is a kind of lake.

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* See Dr. Blake's inaugural Diſſertation. De dentium formatione, p. 110.—1798.

indeed, if it be in large proportion, it communicates a ſenſibly red tinge. *But there is always preſent in the blood, and in ate of ſolution in the ſerum, a quantity of the earthy matter of the bones, phoſphate of lime, ready to be depoſited, as the exigencies of the animal may require. Now the phoſphate of lime is an excellent mordent to madder, and has a ſtrong affinity to it, and is conſequently admirably fitted to afford a baſe for the colouring matter of it: in ſuch experiments, therefore, they concrete in the ſtate of a bright red lake, whence the colour of the bones is derived.* That this is actually the caſe, may be ſhewn by a variety of experiments. Thus, if to an infuſion of madder in diſtilled water, be added a little of the muriate of lime, no change is perceived: but if to this mixture be added a ſolution of the phoſphate of ſoda, immediately a double elective attraction takes place. The muriatic acid combining with the ſoda, remains ſuſpended, or diſſolved in the water; whilſt the phoſphoric acid, thus deprived of its ſoda, combines with the lime which the muriatic acid parted with, and forms phoſphate of lime or earth of bones. This ſubſtance however, being inſoluble in water, falls to the bottom; but having combined at the inſtant of its formation with the colouring matter of the madder, they fall down united into a crimſon lake; preciſely of the ſame tint with that of the bones of young animals, which have been fed with madder. From this ſimple repreſentation of the matter, we have a ready explication of every circumſtance which has been remarked as extraordinary reſpecting this ſubject."

—formed as it ſeems before the oſſeous depoſition.

Dr. Rutherford admits the abſorption and depoſition.

Whilſt Dr. Rutherford thus gives a moſt ſatisfactory explanation of the colour of madder being communicated to the bones alone, of all the white parts of an animal; we find that he embraces the ſame opinion as other phyſiologiſts, that the oſſeous materials acquire their colour previous to their depoſition, whilſt in a ſtate of ſolution or mixture in the blood; from whence they are afterwards depoſited, and concrete in the form of a bright lake. In no part of his ingenious remarks does he hint at the probability that the bones already formed in an animal, may, during the uſe of madder, become red, and after its diſuſe gradually reſume their natural colour, by the agency of a power entirely independent of their depoſition and abſorption: that this is probable I ſhall now proceed to prove.

More particular explanation of the doctrine of the abſorption and regeneration of the parts of animals,

Before it was diſcovered that madder poſſeſſed this property of tinging bones, phyſiologiſts had long been of opinion, that the various parts of the body, being worn out by the performance of their actions and functions, were gradually removed and replaced by new materials. They had ſeen, as Mr. J. Bell obſerves, the whole oſſeous ſyſtem by the morbid removal of its ſolid part, rendered ſo ſoft and flexible as to bend under the common weight of the body and ordinary action of parts; the regeneration of many bones which had been deſtroyed by diſeaſe; the rapid abſorption of fat in ſome diſeaſes, and its ſpeedy reproduction; and laſtly, the gradual change which the fluids of the body undergo, as well as ſome of its inſenſible parts, the hair and nails; hence they ſuppoſed that the ſame proceſs of change and renovation went on in every organ, and that the bodies of animals were not compoſed of the ſame identical particles of which they would conſiſt at ſome future period. This proceſs, which was before but conjectural, or ſupported by analogy, phyſiologiſts conſidered as fully proved by the effects of madder upon the bones. They had by this means an opportunity of ſeeing the bones altered in colour, from the ſlighteſt tint to the deepeſt red; they could obſerve this gradually removed, until the bones had regained their natural whiteneſs; and explaining the whole proceſs on the principle of depoſition and abſorption, they conſidered it as ocular demonſtration of a moſt rapid change in the conſtituent elements of a part, of which, from its ſolidity, they could ſcarcely have believed it ſuſceptible.

—ſuppoſed to be confirmed in the bones.

Probability that this explanation is erroneous.

I apprehend, however, that it is by giving an erroneous explanation of the phenomena; by ſuppoſing that a change in the oſſeous particles is denoted by an alteration in their colour, that phyſiologiſts have conſidered this fact as concluſive.—— However indubitable and well ſupported may be the opinion, which attributes an imperceptible change to the various parts of the body, we ſhall, I believe diſcover upon a more cloſe examination, that it is by no means ſupported by the appearances, which the bones diſplay on the exhibition of madder root. The rapid change in their particles, which ſuch appearances indicate, when explained in the common way, is completely at variance with all the proceſſes performed by the bones, both in their healthy and diſeaſed ſtates. Thus we find the formation of the oſſific matter, called Callus, for the

union

For the proceſſes by which bones are reſtored are ſlow.

union of fractured bones, or the exfoliation of a part of a bone, are proceſſes requiring a conſiderable length of time for their performance. In Dr. M'Donald's experiments, the formation of a regenerated bone required nearly ſix weeks; but during the ſame ſpace of time, the bones of the ſame animal would be renewed ſeveral times, if the common explanation of the communication and diſappearance of the tinge of madder were well founded. From theſe circumſtances, I am led to believe that the appearances produced by the exhibition of madder, require another mode of explanation. That which I have to offer is not liable to the ſame objections, and is ſtrongly ſupported by comparative experiments.

A ſimple explanation grounded on experiment.

It was obſerved by Du Hamel, in his experiments, that the bones of animals which had been deeply tinged by madder, by long expoſure to air loſt their colour and became white.—It was this fact which ſuggeſted to me a ſimple explanation of the proceſs. It occurred to me, that if any one of the component parts of the blood naturally exerted a ſtronger attraction for the colouring matter of madder, than the phoſphate of lime, it might be deprived of the tint by a chemical power.

The ſerum of blood has a ſtronger attraction for the colouring matter of madder than phoſphate of lime has.

In order to prove this, as far as I could by experiment, I took one dram of the phoſphate of lime tinged, as in Dr. Rutherford's experiment, and expoſed it for half an hour to the action of two ounces of freſh ſerum, at the temperature of 98 degrees. By this operation, the ſerum gradually acquired a red tinge, whilſt the phoſphate of lime was proportionably deprived of colour. In a comparative experiment, a ſimilar quantity of tinged phoſphate of lime was expoſed to the action of diſtilled water under ſimilar circumſtances; but no change took place. The knowledge of this ſtrong affinity in the ſerum for colouring matter, affords an eaſy and ſimple explanation of the effects of madder on the bones, upon the principle of chemical attraction.

Hence the bones are dyed when much madder is in the ſyſtem, and bleached by the ſerum when the quantity becomes leſs.

Thus, when an animal has madder mixed with its food, the blood becomes highly charged with it, and imparts the ſuperabundant colouring matter to the phoſphate of lime, contained in the bones already formed; as it circulates through them and moiſtens them throughout. But as ſoon as an animal has ceaſed to receive the madder, and the blood is freed from the colouring matter by the excretions, the ſerum then exerts its ſuperior attraction, and by degrees entirely abſtracts it from the phoſphate of lime, and the bones reſume their natural whiteneſs. In ſhort, the bones are at one time dyed by the colouring matter, at another time bleached by the ſerum.

Phoſphate while ſuſpended does not ſtrongly take the colouring matter.

Whilſt I have attempted to explain the probable manner in which the bones, *already formed* in an animal, at one time receive, and at another are deprived of the colouring matter of madder, I by no means intend to aſſert that the phoſphate of lime does not acquire a ſimilar colour during its ſolution in the ſerum, or at the time it is precipitated from it to enter into the compoſition of the bones; the fact is indiſputable. I have, however, found from ſome experiments lately made upon a hen during oviparation, that only a ſlight tinge can be communicated to the ſhell, formed whilſt a large quantity of colouring matter is circulating with the blood. So ſlight indeed is the bluſh, that it would not be ſeen by a common obſerver, unleſs contraſted with a natural egg: which is probably the reaſon why it has, I believe, been denied by phyſiologiſts, that the ſhell of an egg is altered by the exhibition of madder. If this may be conſidered as a teſt of the quantity of colouring matter, which the phoſphate attracts at the time it is ſeparated from the blood, it forms another ſtrong argument againſt the theory, which Dr. Rutherford, and all preceding phyſiologiſts have adopted; for, conſiſtent with this fact, the bones ſhould never exhibit more than a ſlight bluſh. When explained upon the principle of chemical attraction, we ſee that the phenomena, exhibited by the bones of an animal, by giving or withholding madder root, give no ſupport to the opinion that the various parts of the body continually undergo an imperceptible change; and I conſider it a fortunate circumſtance for that doctrine that ſo ſimple an explanation of the effect of madder can be given. For whilſt ſo ſpecious a fact has been conſidered, by the higheſt authorities, as complete proof of the imperceptible renovation of parts; the rapid change in the conſtituent elements of the bones, which the communication and diſappearance of the colour indicates, muſt have appeared aſtoniſhing to every phyſiologiſt. Of this I cannot give you a ſtronger inſtance than in the words of Mr. J. Bell.* "Nothing," ſays he, "can be more curious than this continual renovation and change of parts even in the hardeſt bones. We are accuſtomed to ſay of the whole body, that it is daily

Example in eggs.

The doctrine of a rapid and continual change is not ſupported by the facts of bones tinged by the madder.

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* Anatomy of the bones, &c. p. 13.

cl ; that the older particles are removed, and new ones their place; that the body is not now the ſame inual body, that it was; but it could not be eaſily believed that we ſpeak only by gueſs concerning the ſofter parts, which we know for certain of the bones.—When madder is given to animals, withheld for ſome time and then given again, the colour appears in their bones, is removed, and appears again with ſuch a ſudden change, as proves a rapidity of depoſition and abſorption exceeding all likelihood or belief; all the bones are tinged in twenty-four hours; in two or three days their colour is very deep, and if the madder be left off but for a few days, the red colour is entirely removed."

Although by this chemical explanation of the effect of madder upon the bones, the doctrine of the imperceptible change in the component parts of animal bodies, loſes the ſupport of a fact, which has, ſince its diſcovery, been univerſally conſidered as its ſtrongeſt proof; nevertheleſs, indiſputable arguments, derived from different ſources, ſtill place that doctrine amongſt the beſt ſupported opinions in phyſiology.

V.

On Fairy Rings and the Waſte of Fiſh in Scotland. By A. T.

To Mr. NICHOLSON.

SIR,

Obſervations and inquiry whether fairy ring may not have been made by hares and rabbits.

HAVING frequently noticed the fairy-rings your correſpondent, M. Florian Jolly mentions in your Journal for February, I ſhould be glad to know from him whether hares or rabbits abounded in Broadlands park, as I have generally obſerved theſe rings moſt prevalent, in light ſandy ſoils, particularly among rabbit burrows. This ſpecies of ſoil from its dryneſs would be very unfavourable to the idea of theſe things being formed from a central heap of horſe dung; beſides, were this the cauſe of them, we ſhould expect them to be always circular, or when not circular, that thoſe parts moſt remote from the centre would appear not to have benefited ſo ſtrongly from the manure as thoſe which were nearer. I have generally obſerved that the rings were compoſed of a double circle, or rather a little circular path, the middle of which appeared to be trodden, and the edges grown up, and more in vigour than any of the ſurrounding graſs. I had occaoſin to remark one of thoſe fairy-rings laſt ſummer: it was perfectly circular, and about ten feet in diameter, it was ſituated at the edge of a copſe wood, and in a vicinity where there are abundance of both hares and rabbits; but what appeared to me moſt ſingular, was its being interſected exactly through the middle, by a well frequented foot path. The hare is rather given to gravity, the rabbit is more playful; but whether it is given to the amuſement of *lounging in the ring*, ſome of your more informed correſpondents may be enabled to inform you.

Fiſh is undoubtedly waſted in Scotland.

I OBSERVE ſome of your correſpondents have got into a controverſy reſpecting the waſte of fiſh in Scotland. No doubt can exiſt upon that head; not however ariſing from the waſteful diſpoſition of the natives, their delicacy in appetite or ſuperabundance of proviſion, but from the want of a market for the conſumption of their overplus. To talk of Aberdeen fiſhermen bringing freſh fiſh to Newcaſtle, Norwich, or Leeds, is as ridiculous as to propoſe taking them to Amſterdam, or London; for beſides the difficulty of again making their own ports, they will conſtantly find an over-ſtocked market, as the ſame weather that permits them to fiſh will permit their neighbours to do the ſame. But the grand cauſe of all the waſte is the horrible monopoly which their country labours under in reſpect to their ſalt laws, where for the ſake of a few paltry pans, Engliſh ſalt is excluded under the ſevereſt penalties, although it can be delivered in any part of Scotland at one half the price that we are forced to pay for Scotch ſalt under the preſent circumſtances. Give them ſalt at a cheap rate, if it does not permit them to export the fiſh, as that requires capital and new eſtabliſhments, it would at leaſt enable them to ſupply the interior; a thing as worthy the attention of the public as the ſupply of any other market I know.

The fiſheries of Scotland are deſtroyed by the operation of the ſalt laws.

Your moſt obedient
A. T.

March 23, 1803.

VI.

Letter from Amicus *reſpecting the ſuppoſed Waſte of Crab-Fiſh in Scotland.*

To Mr. NICHOLSON.

SIR,

Obſervations concerning the fact that the bodies of crab-fiſh are waſted at Arbroath. It is a bad ſpecies which is rejected, the good ones are eaten.

THE very reſpectable and diſtinguiſhed rank which the Philoſophical Journal holds among the periodical publications will at all times prevent its becoming the vehicle of unneceſſary diſpute or contradiction: yet as public information and utility is ſometimes promoted by the correction of miſtakes, when this is likely to be the caſe, any thing that can elucidate a fact either miſrepreſented or partially ſtated, is doubtleſs compatible with the ſpirit of your publication. In your 48th number it is ſtated by "an Enquirer" that the crab fiſhery is ſo productive about Arbroath that, after boiling them, the bodies of the crabs are thrown away, and the large claws only brought to table, of which the Enquirer ſays he has been a witneſs. The fact is literally true, but wants further explanation. It is well known to every perſon reſident on the coaſts where crab-fiſh are commonly to be had, that many of that ſpecies are ſcarcely eatable, being often found after boiling to contain hardly any thing but water. The writer of this article has repeatedly ſeen from twelve to twenty crabs boiled at one time, and every one of them, more or leſs, in the above ſituation. When this is the caſe, the meat of the great claws (although they ſtill may be eaten) is alſo watery and inſipid compared to thoſe of a good crab, the body of which is filled with a very rich ſubſtance, which is ſo far from being thrown away, that it is in general eſteemed a luxury, even where crabs are plenty. Some perſons are, indeed, fond of the claws, who cannot eat the bodies at all; but theſe are only exceptions from general taſte and common practice. The claws of a good crab (as has been already obſerved) are much firmer, more rich, and ſweet to the taſte than thoſe of an inferior kind, which are by far the moſt abundant. The claws of the male are larger in proportion than thoſe of the female: the male crab is alſo reckoned ſuperior in quality, except for a very ſhort period (in what time of the year I have not been able to aſcertain) when in the the opinion of ſome who pretend to be connoiſſeurs, the females are equal, or nearly ſo in that caſe.

Crabs are in ſeaſon nine months in the year; May, June, and July are the only months in which they are not. The piſcatory epicures pretend to certain marks for diſtinguiſhing good crabs, but they are very far from being infallible; perhaps the moſt general diſtinction is, that a good crab has a ſhell of a duſky red colour, with a certain degree of roughneſs, particularly on the claws; while the bad ones have ſhells white, clear, ſmooth, and watery; but the diſtinction is much better underſtood from obſervation than any detailed account. Truſting that you, Mr. Editor, will have the goodneſs to inſert this communication, and that your correſpondent, "the Enquirer," will do me the juſtice to believe that my ſole motive for troubling you was to give information, I am with eſteem, Sir,

Your moſt obedient ſervant,
AMICUS.

Arbroath, March 1, 1806.

VII.

Probability that the Hindoos were acquainted with Saturn's Ring.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of requeſting the inſertion of the following quotation in your Philoſophical Journal, from the 6th vol. of Mr. Maurice's Indian Antiquities, page 605. If it does really mean the ring of the planet Saturn, perhaps ſome of your readers can explain how it could have been diſcovered by the Brahmins in ſuch remote ages.

Your's reſpectfully,
April 7, 1806. A. B. C.

Extract from Maurice's Indian Antiquities.

"I have already intimated in a former volume, that the *circle* formed around Sani (the Saturn of the Hindoos) by intertwining ſerpents, was probably intended to denote his RING. I have ſince had the figure engraved for the reader's inſpection and deciſion. It is impoſſible to aſcertain the exact age of the pictured image in the Pagoda, from which the portrait was taken; but probably both are of a very remote age, for the Indian pagodas are not fabrications of yeſterday, nor in their conceptions and deſigns are they given to frequent viciſſitude. If Sani were thus deſignated in very antient periods, the fact proves that they muſt, by what means can ſcarcely be conjectured, have diſcovered the phenomenon of his *ring*, for what beſides could that ſerpentile oval incloſing the body of Sani be intended to repreſent? That phenomenon however was not known in Europe till about the year 1628, when Galileo, with the firſt perfect teleſcope diſcovered, what he conceived to be two ſtars at the extreme parts of the planet, but which in reality proved to be the *anſæ* of that ring, the natural exiſtence of which was afterwards demonſtrated by Heygens and ſucceeding aſtronomers. The circumſtance is not the leaſt wonderful of thoſe that occur in the diſcuſſion of Indian antiquities and literature. I have ſtated the fact, and engraved the image; I leave to abler judges the taſk of deciſion"

VIII.

Explanation of Time keepers conſtructed by Mr. Thomas Earnſhaw: for which a Reward of Three Thouſand Pounds was awarded by the Commiſſioners of Longitude. From the Communications made by him to the Commiſſioners[*].

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

THE model, from which the annexed drawings were taken contains, beſides the parts neceſſary to explain the nature of the Eſcapement, a box incloſing a ſpring, which when wound up communicates, by means of ſome more wheels, a force to the balance-wheel, ſufficient, when the balance is put in motion, to keep it in action for ſome time. Theſe wheels are contained between two braſs plates, faſtened together by four

upright

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[*] The Eſcapement with a model was communicated in June, 1804, and a ſubſequent explanation in March, 1805. The former is here given, and ſo much of the latter as directly relates to the time-keepers. The latter paper is not otherwiſe abridged than by omitting certain obſervations upon other artiſts, and ſome general remarks which do not form part of the diſcloſure.

I have been ſolicitous to give as early an account as might be proper, of the Eſcapements of Mr. Earnſhaw and Mr. Arnold

which

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

upright pillars; the uppermoſt of theſe plates is that which is repreſented by Fig. 1ſt. plate XIII, where PQRS are the four ſcrews that take into the heads of the four pillars above mentioned, and connect it to the remaining part of the model. The plate PQRS contains, however, the whole of the parts neceſſary for the preſent purpoſe. The ſide of this plate repreſented to view, is the undermoſt when fixed in the model; ſo that the figure repreſents this plate as taken off, with the ſide next to the balance laid upon a table, and the eye is ſuppoſed to be placed perpendicular over it.

In the plate PQRS is an opening, or a piece taken out, repreſented by TUWXYZ. In this opening, the balance-wheel ABCD, pallet MSK, and part of the balance UV are ſeen. The balance-wheel is ſupported by two pieces of braſs, ONH, OI; the piece ONH is ſcrewed to the ſide of the plate neareſt to view by a ſtrong ſcrew *t*, and made firm by ſmall pins repreſented by $\pi\,\pi\,\pi\,\pi\,\pi\,\pi$; theſe pins are called ſteady pins; they are riveted faſt into the ſupporting piece OH, and take into holes in the plate PQRS, made exactly to fit them. The part ON of this ſupporting piece is ſuppoſed to be raiſed above the part *t* H by a joint or bend at N; the other ſupporting piece OI is faſtened to the oppoſite ſide of the plate; and between theſe two pieces the balance-wheel turns freely and ſteadily in the direction of the letters ABCD. The ſmall wheel MSK is called the large pallet; it is a cylindrical piece of ſteel, having a notch or piece cut out of it at *l h r*; againſt the ſide of this notch is a ſquare flat piece of ruby, or any hard ſtone, *h l*, ground and poliſhed very ſmooth, and fixed faſt into the pallet. The cylinder is ſo placed, with reſpect to the balance-wheel, that it may not be more than juſt clear of two adjoining teeth. EF is a long thin ſpring, which

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(which laſt appears in No. 55 of our Journal) as they have been ſo highly diſtinguiſhed by the national munificence. Some diſcuſſion of the important ſubject of time pieces may be ſeen in the Philoſ. Journal, quarto ſeries, Vol. I. 56, and Vol. II. 106. As I expect ſhortly to be favoured with a valuable communication reſpecting the original inventors of free 'Scapements and compenſations, and may, according to circumſtances, offer a few remarks on the ſubject myſelf, I have been careful in the firſt place to give the accounts of the above mentioned artiſts in their own words.

W. N.

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

which is made faſt at one end, by being pinned into a ſtud, G, and made to bear gently againſt the head of an adjuſting ſcrew *m*; the other end is bent a little into the form of a hook; to this ſpring there is fixed another very ſlender ſpring at *γ*, which projects to a ſmall diſtance beyond it. This ſmall ſpring lies on the ſide of the thick ſpring neareſt to the balance-wheel. The adjuſting ſcrew, *m*, takes into a ſmall braſs-cock, *α p*, which is ſcrewed faſt to the plate PQRS by a ſtrong ſcrew at *p*. Upon the ſpring EF there is fixed a ſemi-cylindrical pin, which ſtands up perpendicular upon it, and of a ſufficient length to fall between the teeth of the balance-wheel ABCD. This pin is called the locking-pallet, and is placed on the oppoſite ſide of the ſpring repreſented to view. Through the center of the cylindrical pallet MSK, a ſtrong ſteel axis paſſes called the verge; the pallet is made faſt to this axis, which alſo paſſes through the center of the balance, and is made faſt to it; it has two fine pivots at its extremities, upon which it turns very freely, between two firm ſupporting pieces of braſs ſcrewed firmly, and made as permanent as poſſible, by ſteady pins to the principal plate PQRS; one of theſe pieces is repreſented in the figure by *w y* L; the part *w* is raiſed above the part *y* L by a bend or joint at *n*; the part *y* L being repreſented as fixed firm to the plate by the ſtrong ſcrew at *y*. This piece is called the potence, and is exactly ſimilar to the other ſupporting piece, which is called the cock, that is ſimilarly fixed to the oppoſite ſide of the plate and hid from the ſight in the figure. A little above the cylindrical pallet MSK (as it appears in the figure) is fixed a ſmall cylindrical piece of ſteel *in*, having a ſmall part projecting out at *i*, through which the verge alſo paſſes; this is called the lifting pallet; it fixes upon the verge like a collar, and is made faſt by a twiſt, ſo as to be ſet in any poſition with reſpect to the large pallet MSK. The balance lying below the plate PQRS, only the part UV is repreſented to view; the continuation of the poſition of the circumference, however, is repreſented by the dotted lines ULHV. The end EG of the long ſpring EF being made very ſlender, if a ſmall force be applied at the point *o* to preſs that end out from the wheel ABCD, it eaſily yields in that direction, turning as it were upon a center at G; it is alſo made to ſlide in a groove made in this ſtud in ſuch a manner that the end *o* may be placed at any required diſtance

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

from the center of the verge. Having deſcribed the ſeveral parts as they appear in the figure, we next come to their connexion or ſituation with reſpect to each other. Let the long ſpring EF be ſuppoſed to be ſo placed that the end of the ſlender ſpring *yi* may project a little way over the point of the lifting pallet *in*, but not ſo cloſe but that the point of the pallet may paſs by the hooked end of the ſpring EF without touching it; the head of the adjuſting ſcrew *m* is alſo ſuppoſed to bear gently on the inner ſide of the ſaid ſpring EF, or that neareſt to the wheel, and at the ſame time the locking pallet is ſo placed that one of the teeth D, of the balance-wheel, may juſt take hold of it. This pallet is not viſible in its proper place in the figure, being covered from ſight by the ſcrew *m*, and part of the ſpring EF; its poſition is therefore repreſented by the dot *k*, on the oppoſite ſide of the wheel, having the tooth A juſt bearing up againſt it. From the above deſcription of the ſeveral parts of the eſcapement, and their connexion with each other, it will be eaſy to ſee the mode of its action, which is as follows.

A force being ſuppoſed to be applied to the balance-wheel, ſo as to cauſe it to move round in the direction of the letters ABCD, one of the teeth, as D, will come up againſt the locking pallet (as repreſented at A, and the locking pallet by *k*). The wheel is then ſaid to be locked, being prevented from moving forward by this pin. Let the balance be now ſuppoſed to reſt in its quieſcent poſition, and it will have the ſituation repreſented in the figure; the lifting point *i*, of the pallet *in*, will be juſt clear of the projecting end of the ſlender ſpring, the face *hl* of the large pallet MSK will fall a little below the point of the tooth B, and the balance having its ſpiral or helical ſpring applied to it (which is here ſuppoſed on the other ſide of the plate PQRS, and of courſe not viſible in the figure) remains perfectly at reſt in this poſition. Now as the balance ULHV, and the two pallets MSK and *in*, are fixed faſt to the verge, it is plain they muſt all move together; let therefore the balance be carried a little way round in the direction of the letters VULH; by this motion the end *i* of the lifting pallet *in* will be brought to preſs up againſt the projecting end of the ſlender ſpring, and as this ſpring is fixed on the ſide of the ſpring EF, neareſt to the balance-wheel, the point *i* will preſs the two ſprings together out from the balance-wheel; then, as only the point of the tooth D (ſee its poſition at *k*) touches the locking

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

locking pallet, when the ſpring EF was at reſt again the head of the ſcrew *m*, it will, by the ſpring being preſſed out from the tooth, have ſlipped off (for the locking pallet which was before ſuppoſed at *k*, will now be at *a*, clear of the tooth A of the balance-wheel); the wheel being now at liberty will move round by the force ſuppoſed to be applied to it; but as the point *i* of the lifting pallet moves on and preſſes out the ſpring, the point *l* of the large pallet approaches towards the point of the tooth B of the balance-wheel, ſo that when the ſpring EF is ſufficiently puſhed out to unlock the wheel, the point *l* of the large pallet will be got to *d*, and in this poſition the point of the tooth B of the balance-wheel will fall upon it (ſee *Fig.* 2,) where the tooth B is repreſented in contact with the pallet at *l*; at the ſame time the point of the tooth D has juſt dropt off from the locking pallet *m*; the force of the wheel being by this means applied to the top of the pallet *hl*, gives an increaſed momentum to the balance, and aſſiſts it in its motion in the ſame direction, and by the continued motion of the large pallet in the direction MSK the point of the tooth B, which keeps preſſing and urging it forward, moves up towards the bottom of the face of the pallet towards *h*, until the plain flat ſurfaces of the tooth and pallet come into contact (ſee *Fig* 3); by this time the end *o* of the ſlender ſpring has dropt off from the point *i* of the lifting pallet, and the two ſprings have returned again into their quieſcent poſition, the ſpring EF gently bearing againſt the head of the adjuſting ſcrew *m*, and the locking pallet in a poſition to receive the next tooth C of the balance-wheel; (ſee the poſition of the point of the lifting pallet at *n* *Fig.* 3, alſo the locking pallet at *m*, and the approaching tooth at C.) When the two ſurfaces of the tooth and pallet are thus in contact, the greateſt force of the wheel is exerted upon the pallet, and of courſe upon the balance moving with it. The tooth ſtill preſſing againſt the face of the pallet, and the pallet moving in the direction MSK, it at laſt drops off, (ſee *Fig* 4, where *m* repreſents the poſition of the locking pallet, C the poſition of the tooth of the wheel juſt before it drops upon it, and *l h* the poſition of the face of the large pallet, having the point of the tooth B juſt ready to leave it at *l*,) leaving the balance at perfect liberty to move on in the ſame direction in which it was going. Juſt as the point of the tooth B, which has been preſſing the large pallet round, is ready to leave it,

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

the next tooth C of the wheel is almoſt in contact with the locking pallet *m* (ſee *Fig.* 4) ſo that the inſtant the tooth B drops off the wheel is again locked, and the action of that tooth upon the balance is finiſhed. As the balance moves with the greateſt freedom upon its pivots, the force of the tooth has given it a conſiderable velocity, ſo that the balance ſtill keeps moving on in the ſame direction, after the preſſure of the tooth is removed by ſlipping off from the pallet, until the force of the pendulum ſpring (which is not repreſented in the figure) being continually increaſed by being wound up, overcomes the momentum of the balance, which, for an inſtant of time, is then ſtationary, but immediately returns by the action of the pendulum ſpring, which exerts a conſiderable force upon it in unwinding itſelf. As the balance returns, the point *i* of the lifting pallet *in* paſſes by the ends of the two ſprings EF and *yo*, and, in paſſing by, puſhes the projecting end, *o*, of the ſlender ſpring in towards the balance-wheel, until it has paſſed it; which, as ſoon as it has done, the projecting end *o* again returns and applies itſelf cloſe to the hooked end of the ſpring EF, as before. The ſpring *yo* is made ſo ſlender, that it gives but little reſiſtance to the balance, during the time the point *i* of the lifting pallet is paſſing it, and of courſe cauſes but little (if any) decreaſe in its momentum. During the time the point *i* of the lifting pallet is paſſing the ſmall ſpring *yo*, the long ſpring EF remains ſteadily bearing againſt the head of the adjuſting ſcrew *m*, as the hooked end at *o* juſt lets the end of the lifting pallet paſs by without touching of it. As the ſpring has now been continually acting upon the balance, from the extremity of its vibration in the direction MSK, it has given it the greateſt velocity, when the point *i* of the lifting pallet is paſſing the end *o* of the ſlender ſpring; for at this inſtant the ſpring which was wound up by the contrary direction of the balance, is now unwound again, or in the ſame ſtate as it was in its quieſcent poſition at firſt, and of courſe has no effect upon the balance at all in either direction; but the balance having now all the velocity it could acquire from the unwinding of the ſpring, goes on in the direction UVHL, until the force of this ſpring again ſtops it and brings it back again, moving in the ſame direction as at firſt, with a conſiderable velocity. By this return of the balance, the point *i* of the lifting pallet comes up again to the projecting end *o* of the ſlender ſpring, puſhes back the long ſpring EF, and unlocks the wheel; and another tooth falling upon the face of the pallet *hl* gives freſh energy to the balance: and thus the action is carried on as before.

Inſtructions for making the Eſcapement.

The Eſcapement ſhould be made in the following manner: The pivots of the balance axis ſhould be the ſize of the verge-pivots of a good common ſized pocket watch, and of the ſhape of *Fig*. 5. Pl. XIII, which will greatly add to their ſtrength, the extreme end, or acting part only being ſtraight; the jewel hole ſhould be as ſhallow as poſſible, ſo as not to endanger cutting the pivot, and the part of action of the hole made quite back with only a very ſhallow chamfer behind to retain the oil; deep holes are very bad, for when the oil becomes glutinous, it will make the pivots ſtick ſo as to prevent the balance from its uſual vibration. The pallet ſhould be half the diameter of the wheel, or a little larger, for if ſmaller, the wheel will then have too much action on it, which will increaſe friction moſt conſiderably, and likewiſe cauſe the balance to ſwing ſo much farther to clear the wheel; conſequently a check in the motion of the balance may ſtop the watch. The face of the pallet ſhould run in a line of equal diſtance between the centre of the pallet and its extremity, and not in a right line to its centre, that is an increaſe of friction, and a loſs of that power which is obtained by the wheel acting on the extremity of the pallet: this is clearly proved by time, by the hole worn by the points of teeth in all pallets that run in a line to the centre. The ſcape wheel teeth ſhould form the ſame direction as the face of the pallet, under cut for the ſaid purpoſe of avoiding friction, and maintaining the power, and for ſafe locking. The points of the wheel teeth muſt not be rounded off, but left as ſharp as poſſible. The pivots of the ſcape wheel are to be a very little larger than the balance pivots.

Detent with a ſpring joint.

The wheel is locked by a ſpring inſtead of a detent with pivots, as the French have made them, for thoſe pivots muſt have oil, and when the oil thickens then the ſpring of the pivot detents is ſo affected by it as to prevent the detent from falling into the wheel quick enough, the conſequence of this is irregular time and ſtoppage of the watch, and if ever ſuch a watch went well for twelve months, chance muſt have had by far the moſt hand in it.

How to place the detent, pallets, &c.

When the ſpring is planted on the ſide of the wheel, as in my eſcapements, the part on which the wheel reſts ſhould be a little a little ſhort of a right angle, ſo that the wheel may have a tendency to draw the ſpring into it, for if ſtopped the other way, or beyond a right angle it will have a tendency to puſh the ſpring out; in that caſe the wheel will have liberty to run: the wheel ſhould take no more hold on the ſpring than juſt ſufficient to ſtop it, for if more, friction will be increaſed. The ſmall return ſpring ſhould be as thin as poſſible at the end faſtened to the other ſpring, but at the outer end a little thicker; the ſpring ſhould be planted down as cloſe to the wheel as to be juſt free of it. The diſcharging pallet about one-third, or near one-half the ſize of the large or main pallet, the face of it in a right line to the centre, the back of it a little rounding and off from the centre. Great care muſt be uſed in taking off the edges of this diſcharging piece, to make it round to prevent cutting the ſpring, nor can it be made too thin ſo it does not cut; the end of it neareſt the ballance ſhould be a little more out from the centre of the ballance axis than the lower part of it towards the potence, for counteracting the natural tendency of the ſpring downwards from the preſſure of the ſcape wheel; and that part of the ſpring on which the wheel reſts ſhould be ſtopped a little down to give the wheel a tendency to force it up, to counteract the natural inclination the wheel has to draw it down by its preſſure on it.

Conſtruction of the balance with its compenſation weights, &c.

The balance is to be made of the beſt ſteel, and turned from its own centre to its proper ſize, then put it into a crucible with as much of the beſt braſs as when melted will cover it. The braſs melted will adhere to the ſteel (for if any other metal is uſed by way of ſolder, that watch cannot go well), then turn it to its proper thickneſs, and hollow it out ſo as to leave the ſteel rim about the thickneſs of a repeating ſpring to a ſmall ſized repeating watch, turn the braſs to twice or near three times that thickneſs of ſteel, croſs it out with only one arm ſtraight acroſs the centre, and at each end of the arm fix two ſcrews oppoſite to each other through the rim of the balance to regulate the watch to time, the diameter of the heads of theſe ſcrews about equal to the thickneſs of the balance, a little more or leſs is not material. The compenſation weights ſhould be made of the beſt braſs and well hammered, and a groove turned to let the rim of balance into it, and this ſhould be cut into fourteen equal parts by a wheel engine,

gine, then you will have ſeven pair of pieces of equal ſize and weight; two of theſe pieces being ſcrewed on the rim of the balance at equal diſtances will produce an equilibrium, a balance in the full ſenſe of the word, equal in all its parts. In making balances great care ſhould be taken that they get no bruiſes or bendings, for if they get a bruiſe on one ſide ſo as to indent the metal, that part will be leſs affected by heat and cold than the other parts which have not received the ſame violence to cloſe its pores.

To adjuſt the balance in heat and cold—put the watch into about 85 or 90 degrees of heat, by the common thermometer, mark down exactly how much it gains or loſes in 12 hours, then put it into as ſevere cold as you can get for 12 hours, and if it gains one minute more in 12 hours in cold than in heat, move the compenſation weights farther from the arm of the balance about $\frac{1}{8}$ of inch, and if it gains one minute more in 12 hours in heat than in cold move the weights $\frac{1}{8}$ of inch nearer to the arm of the balance, and ſo on in the like proportion, trying it again and again till you find the watch go the ſame in whatever change of heat or cold you put it,

Adjuſtment to keep time in all temperatures,

Much difficulty has fallen to the lot of watchmakers in the endeavour to make timekeepers go nearly the ſame in the different poſitions. I have had my ſhare of this, but it is now over; by far the greateſt part of this difficulty ariſes from the balance ſpring not being properly made. But if the ſpring is made, as I ſhall deſcribe hereafter, you have only to make the balance of equal weight and it will go within a few ſeconds per day in all poſitions alike, and if it vibrates not more than one circle and a ¼, by applying a ſmall matter of weight to that part of the balance which is downward when in the poſition that it loſes moſt, will correct it with great accuracy; but if it vibrates more than one circle and a ¼, then it will require the weight to be above inſtead of below; and after the watch has been going a few months and its vibration ſhortens to one ¼ circle, then it will go worſe and worſe by reaſon of the weight being in the wrong place; therefore, to avoid this evil, it is abſolutely neceſſary to confine the vibrations to one ¼ circle, which will produce the moſt ſteady performance. It is common for watchmakers to adore a timekeeper when they ſee it vibrate a circle and a half, or more, and form an opinion of its excellence from this only; but I know

—and in all poſitions,

Rule. If the balance be in poiſe it will go nearly alike in all poſitions. Correct it by adding weight to the loweſt part when in loſing poſition if the ſame vibration be about 200°; if more degrees then add to the upper part. Hence a moderate vibration is beſt.

know from experience what would be the conſequence, and have been condemned, becauſe, when I have ſeen ſuch watches I ſaid I ſaw enough to declare that it would not give very accurate performance.

Concerning the balance ſpring.

It is made tapering.

Springs are ſubject to a relaxation of force [illegible] is regained by reſt.

Balance ſpring. To find out the inviſible properties of this apparent ſimple part of the machine, has given much more trouble than all the reſt. I deſpaired of bringing timekeepers to the ſtate I have done, and unleſs thoſe hidden properties are known to timekeeper makers, however well they may execute all other parts they will find their moſt ſanguine expectations fruſtrated. I have ſeen watchmakers boaſt of their timekeepers going well for a month or two, and from the knowledge I had of the effects produced by the balance ſpring, I have told them that a month or two more would deſtroy their hopes. The cylindrical ſpring being in all its turns of equal diſtance from the centre, in courſe every turn will be of equal ſtrength, and called iſochronal, and believed that all vibrations whether long or ſhort would be performed in the ſame time; but this is not true, for if a man is to go four miles in the ſame time as he has gone one mile, he cannot do it with the ſame power; no, he muſt have impelling force to quicken his motion, or he will be four times as long in doing it. Therefore inſtead of the ſpring being equal in all its parts, it muſt be made to increaſe in thickneſs to the outer end, in ſuch proportion as will cauſe the balance when thrown to a greater diſtance to return ſo much the quicker to make them equal; by long perſeverance I found how to make ſuch ſprings, and then I thought I had got all I wiſhed for. But cruel diſappointment nearly broke my heart, for I found I had yet another difficulty to break down, as my watches with ſuch perfect ſprings were continually loſing on their rates. What farther to do I knew not, and I own I was nearly if not quite mad. But obſtinate in the cauſe and reſolving not to give it up but with life, perſeverance came once more to my aid—and with ſtill more unremitting ſtudy, which nearly finiſhed me, before I applied the following remedy for the before mentioned evil. I found, in the courſe of reaſoning on bodies, that watch ſprings relax and tire like the human frame, when kept conſtantly in motion, and this may be proved by the following experiment: let a watch that has been going a few months go down, let it be down for a week or two, or more, then ſet it going, and if it be a good timekeeper ſo as not to be affected by the weather it will go ſome ſeconds per day faſter than it did when it was let down, but it will again loſe its quickneſs in a gradual manner gaining leſs and leſs till it comes to its former rate. Therefore finding that iſochronal ſprings would not do—and likewiſe having made ſprings of ſuch ſhape as would render long and ſhort vibrations equal in time—conſtantly loſe the longer the watch went, I then made them of ſuch ſhape as to gain in the ſhort vibrations about five or ſix ſeconds per day more than the long ones, this quantity could only be found by long experience, and the way I proved this was to try the rate of the watch with the balance vibrating about ⅓ of circle, then tried its rate vibrating one circle and a ½, and if the ſhort vibrations go ſlower than the long ones that watch will loſe on its rate, and if they are equal, it will likewiſe loſe, but that only from relaxation, and if it gains in the ſhort vibrations more than five or ſix ſeconds in twenty-four hours it will in the long run gain on its rate, but if not more than that quantity, and the timekeeper is perfect in heat and cold and every other part, the above properties will render it deſerving of the name of a perfect timekeeper, and this is a principal cauſe of my timekeepers excelling all others, and this the principal cauſe of ſome of my timekeepers going better than others, though made by me, the ſprings of them being made to accord more exactly to the above proportions; and this is the cauſe which has enabled me to foretel what my timekeepers would do, which Dr. Maſkelyne, Mr. Croſley, and others can teſtify. The above effect is produced as follows. I find the common relaxation of balance ſprings to be about five or ſix ſeconds per day on their rates in the courſe of a year, therefore if the ſhort vibrations are made by the ſhape of the ſpring to go about that quantity faſter than the long ones, and as the ſpring relaxes in going by time ſo the watch accumulates in dirt and thickening of the oil which ſhortens the vibrations, the ſhort ones then being quicker, compenſates for the evil of relaxation of the balance ſpring. From this it is plain, that the cauſes of error in timekeepers are not undefined and vague in their nature, which has been ſuppoſed; for when it is certain that all cauſes of error may be over compenſated we cannot deſpair of finding the medium, and which

This gradual effect cauſes a loſs on the rate which may be compenſated by giving greater ſpeed to the ſhorter vibrations in the firſt conſtruction.

which may be eaſily proved by examining the going of my timekeepers. It will there appear that what errors, they are ſubject to, ariſe from cauſes certain and natural, and in courſe may be corrected by art*.

IX.

Experimental Enquiry into the Proportion of the ſeveral Gaſes or Elaſtic Fluids, conſtituting the Atmoſphere. By JOHN DALTON.†

On the component parts of the atmoſphere.

IN a former paper which I ſubmitted to this ſociety, "on the conſtitution of mixed gaſes," I adopted ſuch proportions of the ſimple elaſtic fluids to conſtitute the atmoſphere as were then current, not intending to warrant the accuracy of them all, as ſtated in the ſaid paper; my principal object in that eſſay was, to point out the *manner* in which mixed elaſtic fluids exiſt together, and to inſiſt upon what I think a very important and fundamental poſition in the doctrine of ſuch fluids:—namely, that the elaſtic or repulſive power of each particle is confined to thoſe of its own kind; and conſequently the force of ſuch fluid, retained in a given veſſel, or gravitating, is the ſame in a ſeparate as in a mixed ſtate, depending upon its proper denſity and temperature. This principle accords with all experience, and I have no doubt will ſoon be perceived and acknowledged by chemiſts and philoſophers in general; and its application will elucidate a variety of facts, which are otherwiſe involved in obſcurity.

Objects of this eſſay.

The objects of the preſent eſſay are,

1. To determine the weight of each ſep. atmoſphere.

1. To determine the weight of each ſimple atmoſphere, abſtractedly; or, in other words, what part of the weight of the whole compound atmoſphere is due to azote; what to oxygen, &c. &c.

—and the relative weights of of the gaſes at the ſurface of the earth

2. To determine the relative weights of the different gaſes in a given volume of atmoſpheric air, ſuch as it is at the earth's ſurface.

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* To this communication Mr. Earnſhaw has annexed two plates with deſcriptions, ſhewing the parts of his time-piece; all which, except thoſe of the Eſcapement (which we have given) are capable of the ſame variations as thoſe of any other good movements. He aſſerts that the beſt train for time keepers is 18,000; that the ſcape wheel for pocket ones ſhould have 15 teeth, and for box ones 13 teeth.

† Mancheſter Mem. Vol. V.

3. To inveſtigate the proportions of the gaſes to each other, ſuch as they ought to be found at different elevations above the earth's ſurface.

—as well as at different elevations.

To thoſe who conſider the atmoſphere as a chemical compound, theſe *three* objects are but *one*; others, who adopt my hypotheſis, will ſee they are eſſentially diſtinct. With reſpect to the firſt: It is obvious, that, on my hypotheſis, the denſity and elaſtic force of each gas at the earth's ſurface, are the effects of the weight of the atmoſphere of that gas ſolely, the different atmoſpheres not gravitating one upon another. Whence the firſt object will be obtained by aſcertaining what ſhare of elaſtic force is due to each gas in a given volume of the compound atmoſphere; or, which amounts to the ſame thing, by finding how much the given volume is diminiſhed under a conſtant preſſure, by the abſtraction of each of its ingredients ſingly. Thus, if it ſhould appear that by extracting the oxygenous gas from any maſs of atmoſpheric air, the whole was diminiſhed $\frac{1}{5}$ in bulk, ſtill being ſubject to a preſſure of 30 inches of mercury; then it ought to be inferred that the oxygenous atmoſphere preſſes the earth with a force of ſix inches of mercury, &c.

Now each ſingle atmoſphere preſſes by its whole weight; which is meaſured by its ſpring and that by its volume.

Take away one of the gaſes and the loſs of volume repreſents its preſſure and the weight of that atmoſphere.

In order to aſcertain the ſecond point, it will be further neceſſary to obtain the ſpecific gravity of each gas; that is, the relative weights of a given volume of each in a pure ſtate, ſubject to the ſame preſſure and temperature. For the weight of each gas in any given portion of atmoſpheric air, muſt be in the compound ratio of its force and ſpecific gravity.

The weights of each gas in given volume had from ſp. gravity.

With reſpect to the third object, it may be obſerved, that thoſe gaſes which are ſpecifically the heavieſt muſt decreaſe in denſity the quickeſt in aſcending. If the earth's atmoſphere had been a homogeneous elaſtic fluid of the ſame weight it is, but ten times the ſpecific gravity, it might eaſily be demonſtrated that no ſenſible portion of it could have ariſen to the ſummits of the higheſt mountains. On the other hand, an atmoſphere of hydrogenous gas of the ſame weight, would ſupport a column of mercury nearly 29 inches on the ſummit of Mount Blanc.

The proportions at different heights are obtained from the progreſſion with each gas in the ſame manner as in computation for the whole atmoſphere.

The ſeveral gaſes conſtantly found in every portion of atmoſpheric air, and in ſuch quantities as are capable of being appreciated, are azotic, oxygenous, aqueous vapour, and carbonic acid. It is probable that hydrogenous gas alſo is conſtantly preſent; but in ſo ſmall proportion as not to be detected by any teſt we are acquainted with; it muſt therefore be confounded in the large maſs of azotic gas.

1. *On the weight of the Oxygenous and Azotic Atmoſpheres.*

Proceſſes for determining the oxigen in the atmoſphere.
1. with nitrous gas
2. with ſulphuret
3. Exploſion with hidrogen.
4 Expoſure to green ſulphat of iron.
5 Burning phoſphorus.
All produce the ſame reſult.

Various proceſſes have been uſed to determine the quantity of oxygenous gas.

1. The mixture of nitrous gas and air over water.

2. Expoſing the air to liquid ſulphuret of potaſh or lime, with or without agitation.

3. Exploding hydrogen gas and air by electricity.

4. Expoſing the air to a ſolution of green ſulphat or muriat of iron in water, ſtrongly impregnated with nitrous gas.

5. Burning phoſphorus in the air.

In all theſe caſes the oxygen enters into combination and loſes its elaſticity; and if the ſeveral proceſſes be conducted ſkilfully, the reſults are preciſely the ſame from all. In all parts of the earth and at every ſeaſon of the year, the bulk of any given quantity of atmoſpheric air appears to be reduced nearly 21 per cent. by abſtracting its oxygen. This fact, indeed, has not been generally admitted till lately; ſome chemiſts having found, as they apprehended, a great difference in the quantity of oxygen in the air at different times and places; on ſome occaſions 20 per cent. and on others 30, and more of oxygen are ſaid to have been found. This I have no doubt was owing to their not underſtanding the nature of the operation and of the circumſtances influencing it. Indeed it is difficult to ſee, on any hypotheſis, how a diſproportion of the two elements ſhould ever ſubſiſt in the atmoſphere.

The oxigen and azote are not [illegible]ible.

The firſt proceſs with nitrous gas tho' diſcredited, is here perfected.

As the firſt of the proceſſes above-mentioned has been much diſcredited by late authors, and as it appears from my experience to be not only the moſt elegant and expeditious of all the methods hitherto uſed, but alſo as correct as any of them, when properly conducted, I ſhall, on this occaſion, animadvert upon it.

Inſtructions for the proceſs.

Pure nitrous gas.

1. Nitrous gas may be obtained pure by nitric acid diluted with an equal bulk of water poured upon copper or mercury; little or no artificial heat ſhould be applied. The laſt product of gas thus obtained, does not contain any ſenſible portion of azotic gas: at leaſt it may eaſily be got with leſs than two or three *per cent.* of that gas: It is probably nearly free from nitrous oxide alſo, when thus obtained.

2. If 100 meaſures of common air be put to 36 of pure nitrous gas in a tube 3-10th of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 meaſures, and exhibit no ſigns of either oxygenous or nitrous gas.

Mixture 100 air and 36 n. gas in a narrow tube. Reſidue about 80 azote.

3. If 100 meaſures of common air be admitted to 72 of nitrous gas in a wide veſſel over water, ſuch as to form a thin ſtratum of air, and an immediate momentary agitation be uſed, there will, as before, be found 79 or 80 meaſures of pure azotic gas for a reſiduum.

Mixture 100 air and 72 n. gas in a wide veſſel with agitation. Reſidue as before 80 azote.

4. If, in the laſt experiment, *leſs* than 72 meaſures of nitrous gas be uſed, there will be a reſiduum containing oxygenous gas; if *more*, then ſome reſiduary nitrous gas will be found.

Intermediate proportion of n. gas leaves either n. gas or oxigen with the azote.

Theſe facts clearly point out the theory of the proceſs: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former caſe *nitric* acid is the reſult; in the latter *nitrous* acid: but as both theſe may be formed at the ſame time, one part of the oxygen going to *one* of nitrous gas, and another to *two*, the quantity of nitrous gas abſorbed ſhould be variable; from 36 to 72 *per cent.* for common air. This is the principal cauſe of that diverſity which has ſo much appeared in the reſults of chemiſts on this ſubject. In fact, all the gradation in quantity of nitrous gas from 36 to 72 may actually be obſerved with atmoſpheric air of the ſame purity; the wider the tube or veſſel the mixture is made in, the quicker the combination is effected, and the more expoſed to water, the greater is the quantity of *nitrous* acid and the leſs of *nitric* that is formed.

Theory of the proceſs. In the firſt caſe nitric gas was formed; in the latter nitrous.

To uſe nitrous gas for the purpoſe of eudiometry therefore, we muſt attempt to form *nitric acid* or *nitrous* wholly, and without a mixture of the other. Of theſe the former appears from my experiments to be moſt eaſily and moſt accurately effected. In order to this a narrow tube is neceſſary; one that is juſt wide enough to let air paſs water without requiring the tube to be agitated, is beſt. Let little more nitrous gas than is ſufficient to form nitric acid be admitted to the oxygenous gas; let no agitation be uſed; and as ſoon as the diminution appears to be over for a moment let the reſiduary gas be transferred to another tube, and it will remain without any further diminution of conſequence. Then $\frac{7}{13}$ of the loſs will be due

Practical remark. Operate ſo as to form the nitric gas.

to oxygen. The transferring is neceſſary to prevent the nitric acid formed and combined with the water, from abſorbing the remainder of the nitrous gas to form nitrous acid.

Method with ſulphuret.

Sulphuret of lime is a good teſt of the proportion of oxygen in a given mixture, provided the liquid be not more than 20 or 30 per cent. for the gas (atmoſpheric air); if the liquid exceed this, there is a portion of azotic gas imbibed ſomewhat uncertain in quantity.

Volta's method.

Volta's eudiometer is very accurate as well as elegant and expeditious: according to Monge, 100 oxygen require 196 meaſures of hydrogen; according to Davy 192; but from the moſt attentive obſervations of my own, 185 are ſufficient. In atmoſpheric air I always find 60 per cent. diminution when fired with an exceſs of hydrogen; that is, 100 common air with 60 hydrogen, become 100 after the exploſion, and no oxygen is found in the reſiduum; here 21 oxygen take 39 hydrogen.

2. *Of the Weight of the Aqueous Vapour Atmoſphere.*

To find the weight of aqueous vapour in the atmoſphere.

I have, in a former eſſay, (Mancheſter Mem. vol. 5. p. 2, page 559.) given a table of the force of vapour in *vacuo* for every degree of temperature, determined by experiment; and in the ſequel of the eſſay, have ſhewn that the force of vapour in the atmoſphere is the very ſame as in *vacuo*, when they are both at their utmoſt for any given temperature. To find the force of aqueous vapour in the atmoſphere, therefore, we have nothing more to do than to find that degree of cold at which it begins to be condenſed, and oppoſite to it in the table abovementioned, will be found the force of vapour. From the various facts mentioned in the eſſay it is obvious, that vapour contracts no chemical union with any of the gaſes in the atmoſphere; this fact has ſince been enforced in the Annales de Chimie, vol. xlii. by Clement and Deſorme.

M. De Sauſſure found by an excellent experiment, that dry air of 64° will admit ſo much vapour as to increaſe its elaſticity, $\frac{1}{74}$. This I have repeated nearly in his manner, and found a ſimilar reſult. But the table he has given us of aqueous vapour at other temperatures is very far wrong, eſpecially at temperatures diſtant from 64°. The numbers were not the reſult of direct experiment, like the one above. If we could obtain the temperatures of all parts of the earth's ſurface, for any given time, a mean of them would probably be 57° or 58°. Now if we may ſuppoſe the force of vapour equivalent to that of 55°, at a medium, it will, from the table, be = to .443 of mercury; or, nearly $\frac{1}{70}$ of the whole atmoſphere. This it will be perceived is calculated to be the weight of vapour in the whole atmoſphere of the earth. If that incumbent over any place at any time be required, it may be found as directed above.

It is on an average about one ſeventieth.

3. *Of the Weight of the Carbonic Acid Atmoſphere.*

Deduction of the weight of carbonic acid in the atmoſphere about one thouſandth.

From ſome obſervations of Humboldt, I was led to expect about $\frac{1}{100}$ part of the weight of the atmoſphere to be carbonic acid gas: but I ſoon found that the proportion was immenſely over-rated. From repeated experiments, all nearly agreeing in their reſults, and made at different ſeaſons of the year, I have found, that if a glaſs veſſel filled with 102,400 grains of rain water be emptied in the open air, and 125 grains of ſtrong lime water be poured in, and the mouth then cloſed; by ſufficient time and agitation, the whole of the lime water is juſt ſaturated by the acid gas it finds in that volume of air. But 125 grains of the lime water uſed require 70 grain meaſures of carbonic acid gas to ſaturate it: therefore, the 102,400 grain meaſures of common air contain 70 of carbonic acid; or $\frac{1}{1460}$ of the whole. The weight of the carbonic acid atmoſphere then is to that of the whole compound as 1:1460; but the weight of carbonic acid gas in a given portion of air at the earth's ſurface, is nearly $\frac{1}{1000}$ of the whole; becauſe the ſpecific gravity of the gas is 1½ that of common air. I have ſince found that the air in an aſſembly, in which two hundred people had breathed for two hours, with the windows and doors ſhut, contained little more than 1 per cent. of carbonic acid gas.

Having now determined the force with which each atmoſphere preſſes on the earth's ſurface, or in other words, its weight; it remains next to enquire into their ſpecific gravities.

Specific gravities of gaſes.

Theſe may be ſeen in the following Table.

Atmoſpheric air	-	1.000
Azotic gaz	-	.966
Oxygenous gas	-	1.127
Carbonic acid gas	-	1.500
Aqueous vapour	-	.700*
Hydrogenous gas	-	.077*

Kirwan and Lavoiſier are my authorities for theſe numbers; except oxygenous gas and aqueous vapour. For the former I am indebted to Mr. Davy's Chemical Reſearches; his number is ſomething greater than theirs: I prefer it, becauſe, being determined with at leaſt equal attention to accuracy with the others, it has this further claim for credit, that 21 parts of gas of this ſpecific gravity, mixed with 79 parts of azotic gas, make a compound of exactly the ſame ſpecific gravity as the atmoſphere, as they evidently ought to do, ſetting aſide the unfounded notion of their forming a *chemical* compound. The ſpecific gravity of aqueous vapour I have determined myſelf both by analytic and ſynthetic methods, after the manner of De Sauſſure; that is, by abſtracting aqueous vapour of a known force from a given quantity of air, and weighing the water obtained—and admitting a given weight of water to dry air and comparing the loſs with the increaſed elaſticity. De Sauſſure makes the ſpecific gravity to be ,71 or ,75; but he uſed cauſtic alkali as the abſorbent, which would extract the carbonic acid as well as the aqueous vapour from the air. From the experiments of Pictet and Watt, I deduce the ſpecific gravity of aqueous vapour to be ,61 and ,67 reſpectively. Upon the whole, therefore, it is probable that ,7 is very nearly accurate.

We have now ſufficient data to form tables anſwering to the two firſt objects of our enquiry.

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* The ſpecific gravity of hydrogen muſt be rated too low: if 100 oxygen require 185 hydrogen by meaſure, according to this 89 oxygen would require only 11 hydrogen to form water; whereas 85 require 15. Hydrogen ought to be found about $\frac{1}{10}$ part of the weight of common air.

I. *Table of the Weights of the different Gaſes conſtituting the Atmoſphere.*

	Inch of Mercury.
Azotic gas - - - - -	23.36
Oxygenous gas - - - -	6.18
Aqueous vapour - - - -	.44
Carbonic acid gas - - - -	.02
	30.00

Abſolute weights of the different gaſes in the whole atmoſphere.

II *Table of the proportional Weights of the different Gases in a given Volume of Atmoſpheric Air, taken at the Surface of the Earth.*

	per cent.
Azotic gas - - - - -	75.55
Oxygenous gas - - - -	23.32
Aqueous vapour - - - -	1.03*
Carbonic acid gas - - - -	.10
	100,00

Weights of the different gaſes in equal bulks at the earth's ſurface.

III. *On the Proportion of Gases at different Elevations.*

M. Berthollet ſeems to think that the lower ſtrata of the atmoſphere ought to contain more oxygen than the upper, becauſe of the greater ſpecific gravity of oxygenous gas, and the ſlight affinity of the two gases for each other. (See Annal. de Chimie, Tom. 34. page 85.) As I am unable to conceive even the poſſibility of two gaſes being held together by affinity, unleſs their particles unite ſo as to form *one* centre of repulſion out of two or more (in which caſe they become *one* gas) I cannot ſee why rarefaction ſhould either decreaſe or increaſe this ſuppoſed affinity. I have little doubt, however, as to the fact of oxygenous gas obſerving a diminiſhing ratio in aſcending; for, the atmoſpheres being independent on each other, their denſities at different heights muſt be regulated by their ſpecific gravities. Hence, if we take the azotic atmo-

Computation of the proportion of gaſes above the earth's ſurface.

It is not notably different at any acceſſible height.

---

* The proportion of aqueous vapour muſt be underſtood to be variable for any one place: the others are permanent or nearly ſo.

ſphere as a ſtandard, the oxygenous and the carbonic acid will obſerve a decreaſing ratio to it in aſcending, and the aqueous vapour an increaſing one. The ſpecific gravity of oxygenous and azotic gases being as ſeven to ſix nearly, their diminution in denſity will be the ſame at heights reciprocally as their ſpecific gravities. Hence it would be found, that at the height of Mount Blanc (nearly three Engliſh miles) the ratio of oxygenous gas to azotic in a given volume of air, would be nearly as 20 to 80;—conſequently it follows that at any ordinary heights the difference in the proportions will be ſcarcely if it all perceptible*.

X.

Obſervation which indicates a ſpontaneous Decompoſition of nitrous Acid and Formation of Ammonia. By D. A.

To Mr. NICHOLSON.

SIR,

Decompoſition of nitrous acid.

I SEND you a ſtatement of the following fact, in caſe it may not hitherto have been obſerved: it ſeems to ſhew the mutual decompoſition of nitrous acid and atmoſpheric air; but the explanation of the theory I will leave to you, or ſome of your learned correſpondents. A phial of bright orange coloured nitrous acid, ſo looſely ſtopped that bubbles of gas eſcaped every five or ten minutes, having ſtood within a few inches of a bottle of muriatic acid, cloſely ſtopped for above a twelvemonth, my attention was attracted by obſerving a white incruſtation of ſalts upon the label paper of the laſt mentioned phial. To determine their nature, diſſolved them in diſtilled water; dropped a little nitric acid in, to ſaturate any uncombined alcalies; then with nitrate of ſilver, a copious precipitate was formed, which indicates the muriatic to be the acid: when I ſaturated the acids with pure potaſs, the

---

[*] Air brought from the ſummit of Helvelyn, in Cumberland (1100 yards above the ſea—Barometer being 26,60) in July 1804, gave no perceptible difference from the air taken in Mancheſter.—M. Gay-Luſſac determines the conſtitution of air brought from an elevation of four miles to be the ſame as that at the earth's ſurface.

ſmell reſembled ammonia; but owing to the ſolution being ſo extremely weak, was ſcarce perceptible; but on a finger being dipped into it, and held near a ſtopper, moiſtened with muriatic acid, evidently produced a white cloud, which diſappeared upon withdrawing the acid, and re-appeared on its approach; which left alone I think may be ſufficient to prove ammonia to have been the baſe. I may obſerve, that the ſalts were formed only on that part of the table on which ſome muriatic acid had been ſpilt; the neck of the nitrous acid phial was covered with a moiſture, which had a conſiderable ammoniacal ſmell, and exhibited the ſame appearances with the moiſtened ſtopper, and was therefore uncombined ammonia, and ſeems to ſhew that the preſence of the muriatic acid was not neceſſary for its formation. I have endeavoured to be as conciſe as poſſible, and remain

Your conſtant reader,

April 17, 1806. D. A.

SCIENTIFIC NEWS.

*Note on the Porcelain of Reaumur Communicated by Veau de Launai**.

Reaumur's porcelain made in an improved ſtate.

M. PECARD of Tours, manufacturer of Rouen ſtone ware, has repeateded in his furnace Reaumur's experiment of transforming glaſs into porcelain; mentioned in the memoir of the Academy of Sciences, for the year 1739, p. 370. M. Pecard obtained a devitrification as complete within as without. His experiment was made upon a common glaſs bottle from the Ancenis Foundry. The bottle was filled with Nevers ſand, and depoſited in a *ſagger*, which was afterwards filled up with the ſame ſort of ſand. The ſagger or caſe was placed with others, containing earthenware in the chimney or upper part of the furnace, and heated as uſual. When the operation was finiſhed, and the furnace was ſufficiently cooled, the bottle was taken from its bed of ſand in the ſagger, and emptied of its contents. The bottle had undergone no alteration of ſhape; but its green colour and tranſparency were exchanged for milky opacity, equally ſpread over all parts of the bottle. In this, his firſt experiment, M. Pecard has obtained a much more equal devitrification than that procured by Reaumur; who remarks in his memoir, that he thinks it not impoſſible that this point of equality between the internal and external parts may be obtained.

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* Journal de Phiſique, Vol. LXI. p. 401.

This ſubſtance is much harder than glaſs; it readily gives a ſpark with ſteel; and from the advantages it ſeems to hold forth in many reſpects deſerves to be made an object of inveſtigation.

Darcet applies the glaſs porcelain to uſeful purpoſes.

A diſtinguiſhed chemiſt, who purſues the ſteps of his father, whoſe name will be ever dear to the ſciences, and to thoſe who cultivate them, M. Darcet, has already made ſeveral experiments on this intereſting ſubject, which form part of a work not yet completed. He has made mullers of this ſubſtance, exceeding the hardneſs of flint; alſo capſules and other articles which eaſily ſupport the fire, and are not ſubject to the power of re-agents, ſuch as ſulphuric acid, &c. The little coſt of the materials whereof theſe veſſels, &c. are fabricated, induce a hope that the labours of Reaumur on this ſubject will be reſumed, and carried on in a way that will be of utility in different arts.

Anatomical Work.

AN extenſive work on the anatomy of the organs of hearing in animals, generally, together with the phyſiology of their ſeveral parts, and a ſeries of accouſtic phenomena intended to elucidate the ſubject, is in forwardneſs for publication this ſpring by Anthony Carliſle, F. R. S. F. L. S. and ſurgeon to the Weſtminſter Hoſpital.

The C[illegible] Eng[illegible]

Fig 1

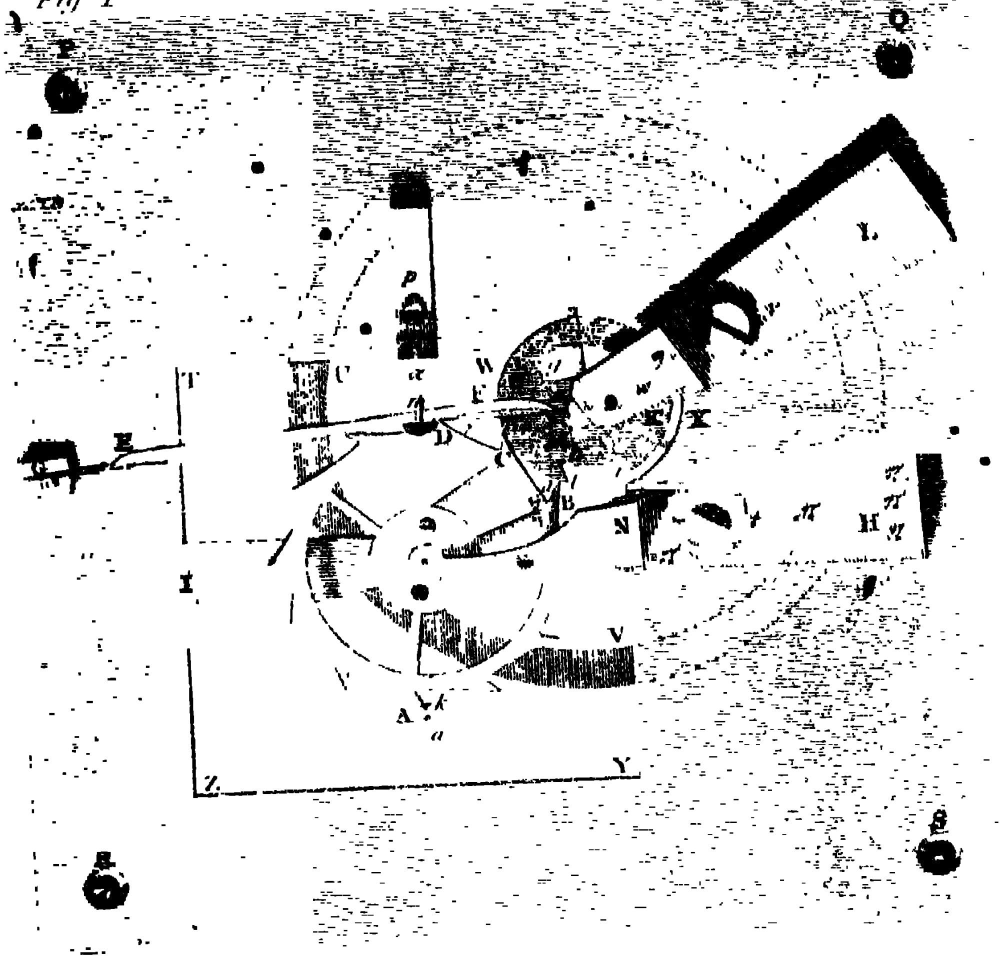

Fig 2

Fig 3

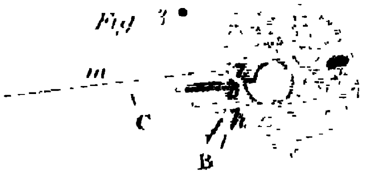

Fig 4

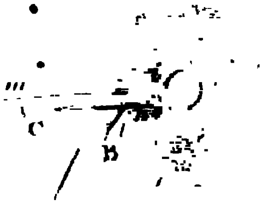

Fig 5

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